

THE PSYCHOLOGICAL REVIEW.

THE 'KNOWER' IN PSYCHOLOGY.¹

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Gentlemen: In the presidential address which I have the honor to read to-day before your Association, I have taken up a psychological problem which seems to me of importance both to psychology and to epistemology, and one which has not, I think, in the general advance of the science of psychology, been treated with the same clearness, or had applied to it the same scientific method, that has led to such good results elsewhere. I allude to the problem of the self or 'knower' as contrasted with those problems arising in the consideration of 'things known,' whether phenomena or 'external' things. I am not here directly concerned with the question of the so-called 'empirical' self, that psychical complex which has been analyzed and discussed much as one analyses and discusses any other mental content. It may, it is true, be difficult to enumerate the elements of which this is composed, but the attitude of the psychologist toward it is sufficiently definite, and the only mystery that the subject presents is the mystery of incomplete knowledge. In discussing it the psychologist at least means something. He applies the scientific method, aiming at and hoping for clear and exact results. The self with which I am concerned is the knower, that indefinite something to which attaches, not merely the mystery of incomplete knowledge, but also, as I cannot but believe, the mystery of misconception; it is that elusive

¹ President's address before the American Psychological Association, Boston Meeting, December 30, 1896.

entity so generally cherished by philosopher and psychologist, which hides itself in clouds and darkness, and whose incomprehensible attributes are accepted without protest by a faith which rests upon tradition and custom.

My statement that I am concerned with the self as knower and not directly with the empirical self commonly said to be 'known' needs a word of explanation, and this I may insert here, though I think my meaning will be made sufficiently clear during the course of my address. I am interested in the question of what *knowledge* means in psychology, and I discuss the self as the accepted subject of the act of knowing. Of course, any light which may be cast on the nature of knowledge will help to make clear what is meant by speaking of anything as 'known,' and will help us to a better comprehension of the 'empirical' self in so far as it is a something known. Moreover, since the self as knower and the self as known have been and are generally very loosely distinguished from one another and even declared identical, it will be impossible for me to confine myself strictly to the self as knower. I must take the self as I find it, vague, ambiguous, inconsistent, and must simply try to come to some conclusion about its 'knowing' function.

So much for my aim. I shall try to remain so far as possible on psychological ground in my discussion, although the matter is, as I have said, also of interest to the epistemologist. One approaches such a theme, in the presence of this critical audience, with a certain reverent hesitation, and would gladly pour out a libation, praying, as did Plotinus, for the gift of correct discernment.

In a paper which I read three years ago before this Association I tried to make clear the nature of the work done by the psychologist, and to set forth the assumptions upon which he must proceed and the method he must employ. I maintained that he must assume the existence of an external physical world, and the existence of certain copies or representatives of it intimately related to particular bodily organisms. These transcripts of the external world, as I expressed it in a later paper, supplemented by certain elements not supposed to have their prototypes without (feelings of pleasure and pain etc.,) are, for

the psychologist, minds. He must by applying the method of introspection, observation and experiment, strive to obtain a knowledge of such minds and reduce their phenomena to laws. He must conceive each mind or consciousness as shut up to its own representations of things and dependent upon messages conveyed to it from without. Ideas are to him, like images in a mirror, numerically distinct from the things which they represent and of which they give information. I further indicated that *knowledge* must, to the psychologist, be a mental state of some kind, a complex in consciousness, and it follows that it must be studied by the usual psychological method. I maintained, finally, that psychology as natural science should resolutely confine itself to mental phenomena, and eschew all such metaphysical entitles as 'substrata,' 'unit-beings,' or 'transcendental selves.' I added that whether one conceive of conscious states as 'parallel' to brain states, or conceive of them as belonging with these latter to the one series of causes, and determining physical movements, in either case one may study them from the natural science point of view. They are in any case phenomena which may be analyzed and described, and the relations of which to other phenomena may be determined by accepted scientific methods.¹

A mind is, therefore, to the psychologist, a little world in itself, cut off from others, and 'knowing' them only through their representatives in it. It is, moreover, a very complex world, and the phenomena it presents are by no means easy to analyze and classify. Some things in it seem to stand out clearly; some remain after our best efforts dim and vague. It is quite conceivable that some things supposed to live and move and have a real being in this world are mere chimæras, having no existence at all except in the imagination, where they lead a dubious existence rather as symbols of the unknown than

¹ In the earlier of the two papers alluded to I point out the inconsistency of the psychologist's position. To cut off minds from things, giving them mere copies or representatives, and then to use the method of observation and experiment, as though the observer were directly conscious of his own ideas and at the same time of his own and other men's bodies, is, of course, flatly self-contradictory. The psychologist has, however, the right to use a convenient fiction, and it need not bring him to grief as long as he remains upon psychological ground.

as concrete intelligible representations. It is not difficult, in the obscurity which still covers much of our mental life, to confound one thing with another, to create a phantom, or to seek diligently for the solution of a problem which need never have been raised.

Certain problems the psychologist may, as I have said, set aside at the outset. All metaphysical entities supposed to be beyond consciousness, and to 'underlie' phenomena, he may simply disregard. He is, to restate my former description of his task in perhaps a broader way, concerned with the contents of consciousness, mental phenomena and their interrelations, and whatever else (if there be anything else) sufficiently resembles mental phenomena to be found in a consciousness. He is also concerned with the relations of mental phenomena to the material world, a something which can be observed and experimented upon. His method is scientific and has already been applied with satisfactory results to some of the phenomena in consciousness. It should be, I think, his duty to strive to apply the same method to everything in this realm. If some things in consciousness need to be further studied from another point of view, and by another method, he may safely leave this task to another workman. Still, even if he remain on his own ground, and regard as the proper object of his studies the contents of consciousness and the physical conditions or accompaniments of this or that psychical fact, it is nevertheless possible that he may fall into some such difficulties or perplexities as are indicated in the preceding paragraph. One need not have a very broad acquaintance with psychological doctrines to see that the task of the psychologist is by no means an easy one, and that warring opinions concerning psychical phenomena may be held with great obstinacy and strong conviction.

Of course, when a writer does not accept and hold to the standpoint of the psychologist as I have defined it, there are still other errors into which he may fall. He may wander into the realm of the metaphysician and return with a self which is not the self of consciousness, the psychologist's self at all. He may confuse this with the psychologist's self, and keep talking about two things while he supposes himself to be discussing

only one. He may transport the self into a world in which reasonable explanations of things, couched in intelligible language, will be sought for in vain. He may make of 'knowledge' a something not in consciousness, and yet not out of consciousness; a thing inconsistent, inscrutable, and, I believe, unpsychological. That many writers have been, and that many are, guilty of these things '*et enormia alia*,' it needs little knowledge of the history of speculative thought to reveal. It is because I am compelled to refer to the works of such writers that I have promised only to *try* to keep upon psychological grounds.

But to come to the particular point which I wish to discuss to-day, the psychological problem of the knower and the known. The plain man, who has not gotten beyond the psychology of common life, has always distinguished in some vague way between himself as knower or doer and the objects which he knows or acts upon. The distinction has become crystallized in language and appears to have past current everywhere and at all times. And in the History of Philosophy we find drawn, with more or less clearness, almost from the beginning, the distinction between that which knows, the mind, soul or reason, and the thing known, which may be either an external thing or a psychical state. I do not propose to weary you with an exhaustive examination of the opinions of philosophers, ancient and modern, but a glance at some of them will, I think, prove helpful in the discussion of our problem.

It is difficult to select from such a cloud of witnesses, but I may mention, in passing, among the ancients, Anaxagoras, Democritus, Plato, Aristotle, the Stoics, the Epicureans and the Sceptics, in all of whom the distinction is sufficiently emphasized. Thales doubtless distinguished in an unanalytic way between himself and the object of his knowledge, but in what little we know of his doctrine his ideas upon this subject do not come to the surface. Perhaps the problem of knowledge had not presented itself to him as a problem. With the growth of reflective thought it comes more and more into view, and the knower grows, I can hardly say more definite, but at least more definitely an object of discussion. This it is with Plato and Aristotle, whose distinction between reason and the lower psych-

ical functions has moreover a flavor of the modern distinction between the rational and the empirical self. In Plotinus the soul, or subject of knowledge, has definitely put on the incomprehensible aspect with which later speculation so constantly clothed it. It is not in space; or, rather, it is in space in an unintelligible and inconsistent way; it is all in the whole, and yet all in every part of the body. It is divided because it is in all parts of its body, and undivided because it is in its entirety in every part. With Augustine, who set his stamp so authoritatively upon the thinking of the centuries which succeeded his own, it behaves no better, being still all in the whole and all in every part of the body. It knows itself and what is not itself. Its properties are not related to it as material qualities are to material substance; they share in its substantiality, although it *has* them, and must not be regarded as *being* them. To make this confusion, if possible, worse, Cassiodorus maintains that the soul, which knows things spiritual and material, is, as a whole, in each of its own parts.

Into the tangles of the Scholastic Philosophy I shall not attempt to enter. Suffice it to say we find everywhere a knower and a known, and this knower, which knows both itself and what is not itself, and may even know itself more certainly than it knows external objects, remains throughout a mystery and a perplexity.

In the Modern Philosophy some of the subtleties of scholastic thought disappear, but, until we come to Hume, the problem remains, I think, much what it was before. With Bacon, Hobbes and Descartes the mind is still the knower, and an ill-defined and shadowy knower. With Descartes it knows itself better than it knows external things. Spinoza's position is an odd and very interesting one. The mind is the idea of the body, or that mode in the attribute thought, which corresponds to the body, a parallel mode in the attribute extension. The mind is composed of ideas, and may be called the knowledge of the body. But there is also such a thing as the idea or knowledge of the mind, and this is related to the mind just as the mind is related to the body. Spinoza finds it impossible, it is true, to keep the idea of the mind apart from the mind itself, since they are both

modes in the one attribute and thus melt into one. His doctrine is not consistent, but its purpose is clear. It appears to him that knowledge demands a knower and a known, and he cannot conceive the knower as playing the part of both. He therefore explains the mind's knowledge of itself by splitting it into a fictitious quality, which fades again into unity.¹ It is interesting to note that to Spinoza the mind is composed of ideas; it is not a something distinct from them and behind them. In Locke there appears again the ambiguous double self, the noumenal and the phenomenal. It is the latter which is directly perceived; the former remains 'an uncertain supposition of we know not what.' Berkeley, the Idealist, basing himself upon Locke's conclusions, classifies the objects of human knowledge as ideas of sense, ideas of memory and imagination, the passions and operations of the mind, and the self that perceives all these. Those who are familiar with the 'Principles' will remember that even Berkeley's clear and graceful sentences leave the reader's mind in a hopeless confusion regarding this last object and the nature of its relation to its own ideas.

In his general demolition of the noumenal and the tradition, Hume cast out everything except what we may now call the empirical self, the self as a complex of mental phenomena. He uses the word, to be sure, as it has since been used by others, to cover our whole mental life, and as equivalent to the word mind. He regards the mind as "but a bundle or collection of different perceptions which succeed each other with inconceivable rapidity, and are in a perpetual flux and movement." Spinoza had, as a psychologist, gone nearly as far, but his mediæval realistic metaphysic, and his desire to have in all cases a knower distinct from the thing known, obscured the force of his teachings. Hume himself, who has written on this as on all subjects with acuteness and admirable lucidity, occupies himself chiefly with destructive criticism, and furnishes no answers to the many objections and inquiries which naturally suggest themselves, and which did suggest themselves to his successors in philosophy. He has, however, done much in clearing the ground for a profit-

¹ See my volume, 'The Philosophy of Spinoza,' Note on the Mind and its Knowledge. 2d ed., N. Y. 1894, pp. 317-324.

able discussion of the question. His writings performed, moreover, the signal service of stimulating to a new course of thought Immanuel Kant, the Sage of Königsberg.

We owe it to Kant, that keen, systematic and inconsistent thinker, that the terms phenomenon and noumenon have become household words. This is no small gain. If a man loosely talks about the self as knowing or doing something, and we ask him whether he refers to the noumenal or to the phenomenal self, only to receive the answer that he does not know, we have now the right to refuse him respectful attention. He does not know what he means to say himself, and it is not likely that his words can profit others. Kant, in the *Critique of Pure Reason*, condemns the noumenon to outer darkness, and shuts up psychology to the world of experience, the phenomenal world. He is not, however, content with Hume's 'bundle' of perceptions, but distinguishes between the multiplicity of psychical elements forming the content of consciousness and a something—not a noumenon, but a something in consciousness—an activity, or whatever one may choose to call it, which makes possible the combination of this multiplicity into the unity of a single consciousness. On this depends the consciousness 'I think' which accompanies all my ideas. The empirical self, as a complex of psychical elements, is to be distinguished from this rational self. This doctrine has had and still has so deep an influence, that it is especially worthy of note in any historical study of the self as knower.

Let me now turn to the treatment of this problem by modern psychologists. The necessary limits of such a paper as this of course preclude anything like an exhaustive treatment of the subject, and I must content myself with an examination of the doctrines of but a few writers. I shall, however, try to select those which seem to me fairly representative of the thought of our time. We do not, I think, find among them much that is distinctly new, though we find, as might be expected, modifications of the views to which I have already referred.

Perhaps I should begin with the descendants of Kant (the line of descent runs through Hegel), a rather numerous and aggressive body, who take their psychology seriously, and are apt

to keep one eye on their metaphysics or theology while discussing psychological problems. As a protagonist of these I may take Professor T. H. Green. Mr. Green repudiated the Kantian noumenon and avowedly confined human knowledge to the field of experience, but he did not approve a Humian experience consisting of a bundle of percepts. He found it necessary to assume in experience a principle of synthetic unity; a principle not to be confounded with any of the elements making up the experience, nor subject to their conditions; a principle which, in some fashion, knits together the manifold of sense into an organic unity. "Thus," he writes,¹ "in order that successive feelings may be related objects of experience, even objects related in the way of succession, there must be in consciousness an agent which distinguishes itself from the feelings, uniting them in their severalty, making them equally present in their succession. And so far from this agent being reducible to, or derivable from a succession of feelings, it is the condition of there being such a succession; the condition of the existence of that relation between feelings, as also of those other relations which are not indeed relations between feelings, but which, if they are matter of experience, must have their being in consciousness. If there is such a thing as a connected experience of related objects, there must be operative in consciousness a unifying principle, which not only presents related objects to itself, but at once renders them objects and unites them in relation to each other by this act of presentation; and which is single throughout the experience."

According to this passage, the knowing or distinguishing agent is conscious and self-conscious, is in consciousness, makes a consciousness possible by uniting different elements, and is single throughout the experience. We find elsewhere that this principle is not *in* consciousness but *is* consciousness, and that everything that exists is in it; that it is intelligence; that it is a subject or agent which desires in all the desires of a man and thinks in all his thoughts. Notwithstanding that it is all this, it has, nevertheless, no existence except in the activity which constitutes related phenomena; and it is, in the words of

¹ *Prolegomena to Ethics*, § 32.

the author¹ 'neither in time nor space, immaterial and immovable, eternally one with itself.'

The mere statement of the attributes of Mr. Green's spiritual principle would seem to be sufficient to condemn it. A faith robust enough to remove mountains might well shy at the task of believing that the single subject or agent which desires in all the desires of a man and thinks in all his thoughts, which is conscious and self-conscious, is still only an activity without existence except as it constitutes the objects of experience, and which, though it does not exist in time, is equally present to all stages of a change in conscious experience. Think of it! the activity which constituted my thought of yesterday did not exist yesterday, when my thought did; and the activity which constitutes my thought of to-day does not exist to-day, while my thought does! Both activities are one, for the activity which constitutes objects is 'eternally one with itself.' What can this mean? If the phrase is to be significant at all, must it not mean that the activity in question is 'always' the same activity? and does not 'always' mean 'at all times?' And what, in Heaven's name, is an 'immovable' activity? Moreover, is it fair to a genuine activity, however abnormal, to call it a principle or subject or agent?

But even supposing it possible for an activity to be all that Mr. Green asks it to be, even to be timelessly present at all times, how are we to conceive of such a thing uniting the elements of any possible experience? Shall we merely assume that it has a vague and inscrutable uniting virtue, as opium was once assumed to have a dormitive virtue? Mr. Green gives no hint of the method by which this activity obtains its result. He does not seek light on this point by a direct reference to experience, for he does not even obtain his activity by direct introspection; he obtains it as the result of a labored process which strives to demonstrate that it must be assumed or experience will be seen to be impossible.

I have read Mr. Green's book with a great deal of care, and have tried to read it sympathetically. Of course, those who sympathize with his doctrine will be inclined to think that, as

¹ § 54.

regards the latter point, I have met with indifferent success. I must confess that the book appears to me to be valuable to the psychologist chiefly as a warning. I have not found Mr. Green's utterances, in one sense of the word, incomprehensible. His doctrine is not fundamentally new. He has taken the Kantian unity of apperception, made of it an hypostatized activity, tried to keep it free of space and time relations, and used it as an explanation of the unity of experience, or as I should prefer to say, of consciousness. He has given us the same inconsistent *totum in toto* thing that we find in Plotinus and St. Augustine. He is, however, a Post-Kantian, and he has included this thing in 'experience.'

It would, of course, be unfair to judge of all Neo-Kantians or Neo-Hegelians on the basis of the utterances of even so prominent a member of the school as Mr. Green. Nevertheless, the way of thinking which characterizes the school seems to me much the same in all, and this is a way upon which, I believe, psychology as science should be careful not to enter. It has led our colleague, Professor Dewey, who can write so clearly when he forgets to what school he belongs, to express himself regarding the intuition of self as follows:¹ "We are concerned here especially with what is called self-consciousness, or the knowledge of the self as a universal permanent activity. We must, however, very carefully avoid supposing that self-consciousness is a new and particular kind of knowledge. The self which is the object of intuition is not an object existing ready made, and needing only to have consciousness turn to it, as towards other objects, to be known like them as a separate object. The recognition of self is only the perception of what is involved in every act of knowledge. The self which is known is, as we saw in our study of apperception and retention, the *whole* body of knowledge as returned to and organized into the mind knowing. The self which is known is, in short, the ideal side of that mode of intuition of which we just spoke;² it is their meaning in its unity. It is, also, a more complete stage of intuition, for, while in the final stage of intuition of

¹ *Psychology*, 3d ed. p. 242.

² *I. e.*, Intuition of the World or Nature.

nature we perceive it as a whole of interdependent relations, or as self-related, we have yet to recognize that we leave out of account the intelligence from which these relations proceed. In short, its true existence is in its relation to mind; and in self-consciousness we advance to the perception of mind."

The self as here described is a universal, permanent activity; it is only what is involved in every act of knowledge, and yet is the *whole* body of knowledge; as returned to and organized into the mind knowing—in other words, into the activity involved in every act of knowledge. Moreover, although it is the whole body of knowledge as thus organized and returned, it is the source of the relations obtaining between the objects making up the world of knowledge. Can any one form a clear notion of such a self? Professor Dewey gives the reader little assistance in making plain to himself how the whole body of knowledge can be returned to and organized into a universal, permanent activity; and he leaves unsolved the problem of how an organized whole consisting of things in relation can itself be the source of relations which make it what it is. Surely this is not sense or science. It is not in place in a modern work on psychology. Taken literally the phrases quoted do not convey any meaning; and taken loosely and figuratively they express, I think, quite as much error as truth. The error here is the error of Green; but the language of the extract is more distinctly the phraseology of a school, and further removed from the plain diction of common life and science. This is, I think, an aggravating circumstance.

Another of our colleagues, Professor Baldwin, has placed himself beside Green and Dewey, and, in so far, has abandoned the standpoint of scientific psychology. In his volume on *Feeling and Will*, he does not often, I think, stray far from the path of empirical psychology, though there is sometimes an indefiniteness of expression which leaves me rather in the dark as to his true meaning. The following, however, is unmistakable:¹ "We may well notice that neither the manifoldness nor the unity of feeling could be apprehended as such in the absence of a circumscribing consciousness which, through its own unity,

¹ N. Y., 1894, p. 79.

takes it to be what it is. Suppose we admit that at the beginnings of life the inner state is simply an undifferentiated continuity of sensation; what is it that *feels* or *knows* the subsequent differentiation of parts of this continuity? It cannot be the unity of the continuity itself, for that is now destroyed; it cannot be the differentiated sensations themselves, for there are many. It can only be a unitary subjectivity additional to the unity of the sensory content, *i. e.*, the form of synthetic activity which reduces the many to one in each and all of the stages of mental growth. The relations of ideas as units must be taken up into the unit idea of relation, to express what modern psychology means by apperception."

In the same category with the above we must put Professors Höffding and Murray, and, I fear, also John Stuart Mill. Mill's chapter on the Psychological Theory of Matter as applied to Mind¹ regards consciousness as a 'string of feelings,' and holds it to be an ultimate and incomprehensible fact that a string of feelings can be conscious of itself as a string. In the appendix to the chapter, printed in the later editions of his work, he admits the existence of an inexplicable tie or law, which is a reality, and connects the feelings with each other. The Neo-Kantian will recognize in this the self for which he enters the lists, though he may disapprove of Mill's forms of expression. I am even tempted to include in the list our colleague, Professor James—at least Professor James in one of his moods, for, although he characterizes the phrase 'united by a spiritual principle' as absurd and empty, yet in the same paper he maintains² that "union in consciousness must be made by something, must be brought about; and to have perceived this truth is the great merit of the anti-associationist psychologists." As, however, he also maintains that if there were a 'soul' it might serve as an explanation of this union, possibly it would be as just to class him with those who hold to a noumenal self. Perhaps it would be best not to class him at all, as he appears so undecided as to what he wants. It is clear, however, that he wants *something* to do the knowing.

¹ Examination of Sir William Hamilton's Philosophy.

² 'The Knowing of Things together,' PSYCHOLOGICAL REVIEW, March, 1895.

I do not think that the substratum soul in its bald and uncompromising aspect, the Lockian 'I know not what,' the Kantian noumenon clearly recognized to be noumenal, plays an important part in the psychological thinking of our time. Still, it is possible to modify or dilute this entity and hold to it in a certain indefinite and inconsistent way. I think this is done by our colleague, Professor Ladd, whose valuable writings are justly attracting no small attention among our contemporaries.

I find in Professor Ladd's last two books many signs of a development in what I must consider the right direction. He is evidently gravitating, although with reluctance, toward psychology as science. His utterances may be collected under two heads according as they reveal the position in which he has heretofore been intrenched, or as they indicate the goal toward which he is moving. Let us glance at a few passages, beginning with some of those which fall under the former head.

We are told by Professor Ladd that the final aim of psychology is 'to understand the nature and development, in its relations to other beings, of that unique kind of being which we call the Soul or Mind.'¹ Our author complains that the larger number of those who cultivate psychology as an empirical science habitually regard consciousness, and the phenomena of consciousness, merely 'content-wise.' They overlook or deny the fact that all consciousness and every phenomenon of consciousness, makes the demand to be considered as a form of functioning, and not as mere differentiation of content. All psychic energy is self-activity; it appears in consciousness as the energizing, the conation, the striving, of the same being which comes to look upon itself as attracted to discriminate between this sensation and that, or compelled to feel some bodily pain, or solicited to consider some pleasant thought. Thus all psychic life manifests itself to the subject of that life as being, in one of its fundamental aspects, its own spontaneous activity.² Again: knowing is distinguished from mere imagining, remembering or thinking, in that it involves belief in reality; and psychological

¹ *Philosophy of Mind*, N. Y., 1895, p. 64.

² *Philosophy of Mind*, pp. 85-88. *Psychology, Descriptive and Explanatory*, N. Y., 1895, p. 215.

analysis shows that knowledge is impossible without this rational, metaphysical belief, or metaphysical faith.¹ "The psychological analysis of any state of so-called knowledge," says Professor Ladd,² "of any of those psychoses properly described by the affirmation 'I know,' shows that all knowledge implicates reality, envisaged, inferred, believed in—we do not now stop to inquire into the manner of implication. Especially is this true of every act of so called *self*-knowledge; for the psychologist is simply ignoring what everybody means by the word, unless he understands the reality of the self-knowing and the self-known, the one self, to be involved as an immediate datum of experience."

From the above so much at least is clear: Professor Ladd believes in a unique kind of being called Soul or Mind, and regards all psychic life, every form of consciousness, as the energizing or striving of this being, holding, further, that all this is manifested to this being as its own spontaneous activity. Moreover this being knows itself, and knows itself as a reality. But whether this reality which knows itself and is the subject of all conscious states is itself in consciousness or not remains rather unclear. The statement that knowledge 'involves belief in reality' would certainly, if words are to be taken in their usual senses, indicate that the reality is not immediately given in experience; and the further statement that knowledge 'implicates reality, envisaged, inferred, believed in,' is vagueness itself, and gives little help in clearing up the matter. It is to be regretted that Professor Ladd did not stop at this point to inquire 'as to the manner of the implication,' for he has not made it clear anywhere else. The latter part of the last extract, which makes the reality of the self an immediate datum of experience, should, perhaps, settle the question; for where the reality of a thing is, there it seems reasonable to expect to find the thing also. Yet, on the other hand, it is maintained that to describe self-consciousness, as a *mere* state or *mere* activity of a definite kind, is imperfectly to describe it, and that 'self-knowledge, although it comes as the result of a development, implies a knowing being that knows itself, in an actual indubitable

¹ *Philosophy of Mind*, p. 100. *Psychology*, p. 513.

² *Philosophy of Mind*, p. 63; cf. *Psychology*, pp. 511-517.

experience, really to be."¹ It appears, thus, that we are not to regard the self as either content of consciousness or activity; so that the empirics complained of above for overlooking the aspect of consciousness which makes it a 'form of functioning' would still be in the wrong even if they included this in their treatment of it. They would have accepted, it is true, every aspect and element of consciousness, but would have left out the real being, which knows itself in an act of metaphysical faith really to be. This speaks for something very like a noumenon; and one begins to feel decidedly that one must accept this as Professor Ladd's doctrine when one remembers that in the same chapter with the sentence above quoted he denies knowledge of mere phenomena to be knowledge at all, and maintains that the word phenomenon has absolutely no meaning except as implying some particular being *of* which, and some being *to* which, the phenomenon is. Professor Ladd prefers, it is true, the expression 'real existence' to 'noumenon,' but that is a mere detail. I conclude, then, that our colleague holds to a noumenal self of some sort, which is responsible for the phenomena of consciousness; and yet, turning at this juncture to the end of the chapter, I am again thrown into confusion by the author's summary of the discussion from which I have taken the above sentiments. I there² find that "the peculiar, the only intelligible and indubitable reality which belongs to Mind is its being for itself, by actual functioning of self-consciousness, of cognitive memory, and of thought. Its real being is just this 'for-self-being' (*Für-sich-seyn*). Every mind, by living processes, perpetually constitutes its own being, and knows itself as being real. To be self-conscious, to remember that we were self-conscious, and to think of the self as having, actually or possibly, been self-conscious—this is really to be, as minds are. And no other being is real mental being." This extract, which the author presents as the sum of the whole matter, seems unequivocally to make of the self nothing more than an activity of consciousness, and, whatever that may be, a self-constitutive activity. It smacks strongly of Neo-Kantism.

¹ *Philos. of Mind*, p. 127.

² *Philos. of Mind*, p. 147; cf. *Psychol.* p. 638.

But what now becomes of that object known, which is not merely an object 'for the knowing process'?¹

Although it is difficult to gain from Professor Ladd's writings any clear idea of what the active subject of mental phenomena really is, one may at least guess from certain passages what he is anxious that it should not be. "This active agent," he remarks,² "actually here and now active and knowing itself as active, is indeed no transcendental being, up aloft in the heavens of metaphysics; but then neither is it submerged beneath the slime, or covered with the thin varnish, of purely empirical psychology." It holds, as it seems, a middle course, and combines the properties of a noumenon, a Neo-Kantian self-constitutive activity and an empirical psychosis.

The last mentioned aspect of Professor Ladd's self or agent, and the one which fixes the goal toward which, as it seems to me, he is moving, comes out very clearly in his work on Descriptive and Explanatory Psychology. We there find that knowledge or cognition is only studied by scientific psychology as a complex psychosis;³ that human mental life does not begin with knowledge; that it not only grows *in* knowledge, when knowledge is once attained, but it grows *into* knowledge only when certain conditions are fulfilled.⁴ The truth that all knowledge implies a development has not, we are told, been hitherto sufficiently emphasized by psychologists, for "at first and for a considerable but indefinite time after birth the child has no such development of any faculty as to make knowledge possible. To it there is no 'Thing' known; to it there is no self as an object of knowledge. This is, however, far from affirming that the child has no states of consciousness whatever—no sensations, no mental images, no feelings, no conation and motor consciousness. Even a considerable development of discriminating consciousness, as the inseparable accompaniment and indispensable condition of all mental development, may take place before the first act, or process, worthy to be called knowledge is reached."⁵ It is insisted

¹ *Philos. of Mind*, p. 100. *Psychol.* p. 513.

² *Philos. of Mind*, p. 106.

³ *Psychology*, N. Y., 1895, p. 508.

⁴ P. 509.

⁵ P. 510.

that "all objects of knowledge, psychologically considered, are alike to be regarded as states of consciousness; all states of consciousness are time processes in the on-flowing stream of consciousness. This is as true of the things perceived by the senses as it is of the self known in self-consciousness."¹ Again: "In the earlier stages of mental life no psychoses can be discovered which are worthy to be called a knowing of self."² The gradual development of the psychosis called a knowledge of self, Professor Ladd traces at length, and concludes thus: "Finally, it is by complex synthesis of judgments, based on manifold experiences converging to one conception—the resultant of many acts of memory, imagination, reasoning and naming—that the knowledge of the Self as a Unitary Being is attained." Only at this stage is self-consciousness in its highest sense possible; but in this stage "in one and the same act the mind makes itself the object of its self-knowledge, and believes in the real being of that which it creates as its own object."³

Surely all this is plain and unvarnished empirical psychology, with only a few traces of the old-fashioned rationalistic doctrine. It is psychology as science. But it is very hard to fit it to what has preceded. We find here that in the earlier stages of consciousness there is no self as known. It, of course, follows that during these stages there also exists no self as knowing, no agent, no reality; for is it not true that consciousness regarded as objectively discriminated, and consciousness regarded as discriminating activity, are only two sides of one and the same consciousness?⁴ and are not the self-knowing and the self-known the one self?⁵ and does not the existence of this one self depend upon its actual functioning as self-consciousness? "To be self-conscious, to remember that we were self-conscious, and to think of the self as having, actually or possibly, been self-conscious—this is really to be, as minds are."⁶ There are then sufficiently complex consciousnesses containing sensations,

¹ P. 519.

² P. 523.

³ Pp. 531, 532.

⁴ *Philos. of Mind*, p. 89; *Psychol.* p. 291.

⁵ *Philos. of Mind*, p. 63; cf. *Psychol.* p. 532.

⁶ *Philos. of Mind*, p. 147; cf. *Psychol.* p. 638.

images, feelings, motor impulses, and even a considerable development of discrimination, which are not the manifestation of any reality, or the states of any being. As yet there is no mind or self of which they may be the manifestation. Here are activities without any 'thing' that is active. Here are phenomena without any reality *of* which and *to* which they are the phenomena. We must then abandon the position that all psychic energy is the activity of the self, for the self must be begotten or beget itself before it can act; and we must also reconsider the statement that the word phenomena has no meaning except as implying some particular being *of* which, and some being *to* which, the phenomenon is.

The two elements in Professor Ladd's doctrine cannot, I think, by any possibility, be made to harmonize. It is war to the death; and I believe the careful reader of the earlier and later works of our colleague will see that the issue of the conflict is scarcely a matter of doubt. Professor Ladd's soul as 'envisaged reality' is gradually slipping away from him. I should not be surprised to see him in some later work apostrophizing it after the manner of Hadrian:

"Animula vagula, blandula,
Hospes comesque corporis,
Quae nunc abibis in loca?"

Let us hope that, when it does take its departure, it may find some abode with an atmosphere less rarified than the heaven of the transcendentalists, and let us also hope that it may escape a damp and unpleasant interment in so-called empirical 'slime.'

I have dwelt at length upon Professor Ladd's doctrine both because of our own interest in his work and because it has seemed to me profitable to show into what perplexities even a learned and really scholarly man is in danger of falling, when he wanders from the narrow way of scientific psychological method, and takes to what Diogenes Laertius calls a noble line in Philosophy, dealing with the incomprehensible. It remains for me to say a word concerning those whom I may call the successors of Hume. I think we will all admit that Hume wrote rather crudely concerning the self, and that his 'bundle' of perceptions is by no means able to take its place without

modification in a modern psychological treatise. I do not mean, therefore, in speaking of the successors of Hume, to indicate that those referred to write in the same crude fashion. I only mean to indicate that they have abandoned the traditional self of the History of Philosophy, and have not replaced it by an hypostatized unitary activity in consciousness or in 'experience,' but regard it as the whole task of the psychologist to study the 'content' of consciousness in a broad and reasonable sense of the word content. In this class I place Professor Wundt, as he appears in his later writings;¹ Professor Külpe, who states and maintains more unequivocally than Wundt, Wundt's later psychological doctrine;² Professor Ziehen, who almost succeeds in leaving out of his clear little book on Physiological Psychology, all non-psychological reference; and Professor Titchener, who holds that there is no psychological evidence of a mind which lies behind mental processes, and no psychological evidence of a mental 'activity' above or behind the stream of conscious processes.³ It is interesting to note that these men have approached psychology from the physiological and experimental side; and one is tempted to think that the novelty of their task and the conditions under which they have been compelled to approach it, have somewhat loosened for them the bonds of tradition, and have enabled them to place themselves more completely on the ground proper to psychology as science than it has been possible for a goodly number of their co-workers to do.⁴

It is unnecessary for me to say that I regard their position as the right one, though I should not like to be understood as ap-

¹ Grundzüge, Leipzig, 1893. *Human and Animal Psychology*. London, 1894.

² Grundriss, Leipzig, 1893.

³ *Outline of Psychol.*, N. Y., 1896; p. 341.

⁴ It is proper for me to state that none of these writers have appeared to me to fully appreciate the significance of their own position for the psychological doctrine of 'knowledge.' A commentator always treads upon uncertain ground, but I am inclined to believe that their scientific attitude in treating of the self has really been brought about by the causes to which I have alluded. They have wished to avoid metaphysics and hold to clear psychological concepts. This does not necessarily imply that they have seen the total value of these concepts either for Psychology or Epistemology. I suppose all the writers I have mentioned would give a goodly share of the credit for bringing before the public the doctrine I am discussing to Wundt, although he has not been the most happy in giving its expression.

proving all the details of their treatment of psychological problems. The study of the content of consciousness and of the relations of mental phenomena to the physical world seem to me the proper task of the psychologist as psychologist. And by the words 'content of consciousness,' I do not mean content in the Kantian sense, a something contrasted with 'form;' I mean all that is to be found in consciousness, including relations, changes and activities. But relations, changes and activities should be treated in a scientific and intelligible way. If I have a perception of three black dots on a white surface, so related to one another that lines joining them would form an equilateral triangle, surely the relations of the dots are as much a part of my perception as the color of the dots; and, if I see again on the following day three similar dots similarly related, I am surely not justified in declaring the relations perceived on the two occasions, to be identical in any sense in which the dots are not. If, further, I describe the formation of any psychosis in consciousness to-day as the manifestation of an activity, and the formation of a like psychosis in consciousness to-morrow, as also the manifestation of an activity, surely the two activities should be as carefully distinguished as the psychoses themselves, and each relegated to the particular time at which it manifested itself. The word 'activity' is not a word to conjure with; and when speech ceases to be intelligible, silence is golden. There is nothing in the view of the task of the psychologist which I am advocating, to make him overlook or slight any phenomenon or aspect of consciousness. He is not compelled to regard our mental life as composed of unrelated elements, or to look upon it as passive or mechanical. He need not betake himself to unusual or misleading expressions such as the 'self-compounding' or 'agglomeration' of ideas. He has the same right others have to take language as he finds it, and to do his best with it, striving only to be clear and exact and to avoid being misunderstood. He must recognize that when men say 'I think,' 'I believe,' 'I know,' 'I feel,' 'I will,' 'I remember,' 'I am self-conscious,' these words indicate the presence in consciousness of complex psychoses, which it is his duty to analyze to the best of his ability. His task is not an easy one; and even if he

follow loyally a good method, confining himself resolutely to the field that I have indicated, he may for a long time to come expect to find in it much that cannot be so brought into the light as to make him confident that he has completely analyzed and described it. Notwithstanding all this, he may take comfort in the thought that his method is the true one. Even if the goal be far distant, it is something to be on the right road.

I have no doubt that many will object that this simply abandons the psychological problem of the knower and his knowledge, and does not solve it. They will insist: How can there, after all, be a consciousness, unless something unifies it? can one psychosis know another? or 'a string of feelings' know itself as a string? Where in all this is the knowing? I answer, the psychological problem is indeed abandoned, for it is only through a misconception that such a psychological problem exists at all. How the traditional knowing self came into being and became a perennial stone of stumbling to the speculative mind, it is not, I think, difficult to conjecture; and a brief exposition of what I believe to be the genesis of this self will be the best justification of my statement that the problem has no right to demand a solution.

It is generally accepted among psychologists that, at an early stage of the mind's development, the chief constituent of the notion of the self, and perhaps the only one that stands out with sufficient clearness to occupy the attention, is the idea of the body. When the child says 'I see,' 'I hear,' 'I feel,' he is not thinking of the self of the philosophers, but is recognizing the fact that, given his body in such and such a relation to other objects, he has certain experiences. His body stands over against other objects and is distinguished from them. It sees with its eyes, hears with its ears, feels with its hands. It not only sees, hears, and feels other objects, but also sees, hears and feels itself. It perceives not merely that it is acted upon, but also that it acts upon other things, bringing about changes in them. It is the constant factor in experience, while the objects with which it occupies itself succeed one another in a more or less rapid succession. Moreover, it is an interesting object, with which are bound up in a peculiar manner the pains and

pleasures of the individual. No wonder it becomes the centre of the little world in which it has its being, a world concrete, unreflective, external, if I may be permitted to use this relative word when the correlative can not as yet be regarded as having made its way into the light of clear consciousness—at least a world objective and material in the sense that what comes later to be recognized as objective and material almost wholly constitutes it. And from the crude materialism of the infant mind to the crude animism of the savage the step is but a short one. That duplicate of the body, which in dreams walks abroad, sees and is seen, and acts as the body acts, has simply taken the place of the body as knower and doer, and its knowing and doing obtain their significance in the same experience. The thought of the child is duplicated in the new world opened up by the beginnings of reflection.

Now, I believe that the student of the History of Philosophy who is able to read between the lines can see in the highly abstract and inconsistent '*totum in toto*' soul of Scholasticism, and in the 'transcendental unity of apperception' of Kant, a something that has grown by a process of refinement from these rude beginnings. These nebulous entities do not make their appearance upon the stage unheralded. We find early in the history of thought a material soul which knows things by contact with the effluxes thrown off from material objects. It is an object among other objects, as is the body, and the nature of its knowing is clearly analogous to that of the body's. We have, later, a soul in part fettered to the body, and, as it were, semi-material. We have, finally, a soul abstract and unmeaning, a shade, a survival from a more concrete and unreflective past. It is worthy of note that with this development the soul and its method of knowing become more and more unintelligible. How the soul as noumenon or as super-temporal activity can know anything or do anything, no man can pretend to understand. The reason is not far to seek. In the successive transmutations through which it has passed, almost all reference to the primary experience out of which the notion of a soul or self as knower and doer took its rise has been lost. Were such reference completely lost, it would go hard with the hypostatized abstrac-

tions of the Noumenalist and the Neo-Kantian. As it is, they hold their own because men really do find in their experience something which seems to speak for them in a certain vague and inarticulate way. They can form no conception of the method by which a noumenon or a Neo-Kantian self-activity can account for their experiences, but they prefer these to nothing at all; for must there not be a knower? do *they* not really *know*? Their position is one quite easy to understand. It is not exclusively to the childhood of the individual or of the race that we need go to find the body an important element in the self-idea. The developed man has much the same experience as the child, and instinctively interprets it in the same way, although reflection has furnished him with the means of correcting this instinctive interpretation. Even the psychologist who writes clearly and systematically concerning the empirical self, which he recognizes as nothing more than a complex in consciousness, may retain as a troublesome and inexplicable entity a second self, the knowing self contrasted with the self known—identical with it, and yet distinguished from it; the same, and yet not the same. Here he may revel, as those who have preceded him have reveled, in self-contradictions and unintelligible discourse. He may apply to the self the unhappy title of 'subject-object' and endeavor to separate a thing from itself, positing a relation between the two, when there are not two but one to be related. It requires but a moment of unprejudiced reflection, it seems to me, to see that all this is absurd and unmeaning. The only question of real interest is: How have men come to speak in this way? The answer I have indicated above. When one whose chief idea of the self is the body¹ speaks of perceiving himself among other objects, he has reference to an experience which he and others constantly have; and he has used a certain expression to call attention to that experience. His thought may not be clear and analytic. His statement, if the words be taken quite literally, is meaningless. Still, he means something by it, and it is the duty of the psy-

¹ Of course, I have no intention of taking here any position regarding the body as a material, external thing. Any one who pleases may substitute for the word such expressions as 'experience of the body,' 'relatively permanent organic feelings,' etc.

chologist to show him what he means. It is not his duty to turn an inconsistency of expression into an inconsistency of thought, and find in his words what, in their proper interpretation, they do not contain. Our Noumenalist, or our Neo-Kantian, thus bases himself upon an experience, even though he misinterprets it. He draws from experience the impulse to carry over into a region in which it has no right to exist the notion of a bodily self. He refines it, he purifies it of all that is earthly and concrete, starves it to a shadow of its former self, and yet expects of it its former tale of bricks—knowing and doing.

This I cannot but regard as delusion; as a misinterpretation of our common experience. This path let the psychologist avoid. To him knowledge is a psychosis to be analyzed; so is self-knowledge. The unity of consciousness he may accept as he finds it, striving to make clear to himself what he means by 'unity' in general, and by the unity of consciousness in particular. To attempt to explain the ultimate nature of consciousness by the assumption of hypothetical entities not to be found in consciousness, or by ascribing inconceivable virtues to hypothesized activities, seems to me an unprofitable task.¹

My address is already longer than I intended to make it, and yet I feel with regret that I have not been able to speak on some of the points upon which I have touched, as clearly and fully as I could have wished. Nevertheless, I must beg your indulgence in allowing me to mention very briefly one point more. Psychologists are men, and may be assumed to share the hopes and fears common to men of their degree of intelligence. It is quite possible that some among us have already mentally characterized my position by applying to it the damnatory phrase

¹ In spite of the fact that I heartily dislike seeing a discussion encumbered with foot-notes, I must add one more. I have said at the outset that I would try to confine myself to psychological ground. I have, hence, raised none of the epistemological questions which are suggested by the one question I have been discussing. I have not criticized from the standpoint of epistemology the psychological standpoint, nor asked how one may *know* that there is an external world. I have not asked what it means for two men to know the same thing, or how one consciousness can be known to be outside of another. I have simply discussed the general problem of knowledge and of the knower in psychology, and I have stated the problem in its simplest form. Until some satisfactory solution is given to the problem as thus stated, it seems to me to be futile to attempt the solution of more intricate problems of the same nature.

'psychology without a soul,' and have felt that what I have said militates against the existence of the soul after death. My discussion has, however, left this question just where it was before. It was pointed out by Mill long ago, that if it is possible for a 'string of feelings' to have a continued existence in this life, there can be no *a priori* objection to its having such an existence in another. Even so I would say, if a consciousness can here develop during a period of years, and retain that identity which it is the duty of the psychologist to analyze and describe, there is nothing in a man's repudiation of noumena or supertemporal activities to prevent him from believing that his conscious life may continue indefinitely. My reference to this matter may be a little out of place, for we are here to-day as psychologists, and have before us a definite and limited field of labor. Still, it is hard for men to approach scientific questions without asking what is their bearing upon theological or religious convictions. Perhaps it is right that such questionings should arise. I have added this paragraph in the hope that what I have said may not meet with a prejudice arising out of a mere misunderstanding, and be condemned through the application of a question-begging phrase.

STUDIES IN THE PHYSIOLOGY AND PSYCHOLOGY OF THE TELEGRAPHIC LANGUAGE.

BY PROFESSOR WILLIAM LOWE BRYAN AND MR. NOBLE
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In March, 1893, Mr. Harter began at the Psychological Laboratory of the Indiana University a study of certain problems connected with the acquisition of the telegraphic language. Eleven months were spent at and away from the University in a preliminary study of these problems, in the light of his own experience, and by diligent, personal cross-examination of thirty-seven operators, employed by the Wabash Railway Company and by the Western Union Telegraph Company. Of these, seven were recognized as experts, twenty-two as men of average experience and ability, while eight had barely enough skill to hold their places. Twenty-eight of the number had been personally known to H. from two to sixteen years. Throughout these and the subsequent investigations, the members of the telegraphic fraternity showed the most cordial interest and readiness to help.

In March, 1894, H. began at the University Laboratory, an experimental study of individual differences in telegraphic writing. The experimental part of this study continued until August, 1894, and the study of the results until June, 1895. A review of certain phases of the results was made in January, 1896. During the winter of 1895-96 he was engaged with the study of the curve of improvement in sending and receiving.

II. THE PRELIMINARY STUDY.

The first year's work was in the nature of an exploring expedition in search of the problems which would repay fuller in-

¹N. H., a graduate student of Psychology at Indiana University, was for many years a railroad telegrapher, and is an expert in that branch of telegraphy. The experiments were made under Professor Bryan's direction.

vestigation. It seems best, on the whole, to give a somewhat gossipy, through brief account of this exploration. The method of exploration consisted in cross-examining the operators with questions which, on the one hand, seemed to have psychological or physiological significance, and which, on the other hand, in the light of H's personal experience, gave promise of answers.¹

The student learns to distinguish most of the letters of the telegraphic alphabet in a few hours or days at most; but after distinguishing them clearly at one time, he generally finds himself confused by the back stroke, and must re-learn the letters many times before this difficulty is overcome. The back stroke is the stroke of the armature against the adjusting screw above it. Those letters composed of simple dots or dashes are mastered first, then those composed of dots and spaces, and lastly those most complex, as, J (— . — .), X (. — . .) Q (. . — .).

As the characters composed of four, five and six dots, are made more rapidly than the learner is able to count, much practice is necessary before he can recognize surely the number of dots in such groups. When a considerable degree of speed in receiving is reached, the space between the letters of a word becomes so small that one ceases to recognize it consciously, the letters seem to blend together, and the word is recognized as a sound whole. Thus, expert operators read *words* from their instruments; and, as will be seen later, these group themselves into larger wholes, so that the sentence becomes the conscious unit, much as in the reading of printed matter. Of course, the short and frequently recurring words are the first ones to have their parts melt together. A learner is thus very soon able to distinguish such words as, 'the,' 'is,' 'and,' etc., when written swiftly on the main line in their proper connection, while he cannot understand even a single letter in other words. This observation shows that a given group of sounds, for example those making the letter H, may be apperceived in one instant because occurring within a larger known group, and then not apperceived a moment later because occurring as part of an unknown group.

¹ To save circumlocutions, technical words in common use among telegraph operators and whose meaning is plain, will be used in this article.

There are distinct specialties in telegraphy, so that while an operator may be competent in one department he would be a failure in another department requiring no greater speed, until he had acquired the vocabulary of that department. Thus, a commercial operator would be 'lost' in a yard office, or the train dispatcher in taking markets. It is the opinion of experienced operators that while there are many exceptional cases of quickness and slowness in learning,¹ it requires from two to two and a half years to become an expert operator. Through lack of energy to practice, except when compelled by the nature of their work, few operators reach their maximum, while many have little more than the skill actually required in their daily work. To gain expertness, work increasing in difficulty must be faithfully done. For a fuller discussion of this point see below, part IV.

The effect of sending a long strange word swiftly is to cause the receiver to make an error or break—that is, to ask for a repetition of the word. Many operators are very sensitive on the subject of breaking, and some do not hesitate to supply the most probable word and thus avoid what they consider a humiliation. Sometimes an obnoxiously smart young operator is allured through his pride against breaking, into a trap, whose psychology is significant. The date and address of a message are sent at a rapid rate, followed by the period which separates the address from the body of a message. The letters of the alphabet in order are then plainly but rapidly sent to him. The receiver expects a message. In the first few letters he recognizes no word group. He hopes to see the connection a little farther on. He is finally compelled to break. It is sometimes possible to repeat this trick several times without the victim discovering it. In this case evidently the strenuous effort of attention to recognize word groups has prevented the recognition of a most familiar group, namely, the alphabet in order.

Another interesting apperceptive illusion, to which even ex-

¹ A story is told of an Indiana operator, who, after three months' practice, was able to receive Garfield's Inaugural Address. Stories of this sort must be taken with a grain of salt. The more of telegraphy you know, the more salt it takes.

pert operators are subject, arises in the following way: The adjustment of the armature of the sounder is controlled by set screws so that the down stroke and up stroke may be differentiated. The down strokes, of course, correspond to the dots and dashes of the Morse code. Whether a dot or dash is intended, is determined by the length of time between the down stroke and the following back stroke. It is evidently essential that the down stroke and the back stroke should be clearly distinguishable. Making this distinction is one of the greatest difficulties in learning telegraphy. The student may learn to recognize several of the simple dotted letters in a few minutes; yet after an hour, when *e*, a single dot, is made, he interprets the two sounds which he hears as *i*, which is two dots. Learners seek to assist the ear by watching the teacher's hand or the armature of the sounder. This device must, of course, be prohibited, and the learner required to depend on the ear alone. Several instances have been observed in which expert operators have made the same sort of error when listening to strange instruments in which the down stroke and back stroke were not very clearly different. In such cases the experts were unable to understand even a single letter. By a sort of inversion of attention the back strokes are heard as down strokes, and *vice versa*.

The rate of receiving varies greatly. On train wires, about twenty to twenty-five words, of four letters each, per minute, may be taken as the ordinary rate of communication. Among lower grade operators the ability to send is greater than the ability to receive, but with experts the reverse is generally true. The highest sending record, so far as known, is forty-nine words per minute. When the type-writer is used the ability of the receiver exceeds that of the sender. Words in cipher cannot be received so rapidly or so accurately as ordinary language. The telegraph companies recognize this fact by charging very high rates for combinations of letters forming other than ordinary English words. Errors in the transmission of messages are comparatively few, and it is the common feeling of telegraphers that they could testify more surely in Court concerning what they have heard on the line than concerning what

they have heard from personal interview, or as to the accuracy of a message which they have copied from a sounder than as to one which they have copied from dictation.

External disturbances have a very great effect upon inexperienced operators, but affect the experienced operator very little. It is not uncommon to see an operator doing a large amount of important work in a small room where half a dozen sets of instruments are working, trainmen running in and out, talking excitedly and asking questions, engines moving by the window and trucks running noisily by on the platform. Yet the operator works ahead, calmly and rapidly, and even briefly answers questions addressed to him. Where a number of sounders work close together, pieces of tin, brass or the like may be attached so as to give each a distinguishable tone. Many men can receive from an instrument adjusted low in the presence of others sounding much louder. The ability to do this, however, is much lessened by lack of practice. Dispatcher C. was a copyer for several years in an office where he worked with several instruments close together. After he had been promoted two years to a dispatcher's desk, where but one instrument was used, he lost the ability to do the work he had formerly done, being confused by the working of the other instruments.

Subjective disturbances, as fear, anger, excitement, etc., have little effect on expert men other than to make them more fluent in the use of the telegraphic language. Operators are keenly alive to the presence of those with whom they communicate, so that they do not feel alone, although no one is physically present. This feeling causes young operators to suffer keenly from stage fright, especially when making their debut. The first work generally consists in reporting a train to a dispatcher. The debutant is very anxious to do so, and practices hard so as to do it well, but almost invariably does it in such a manner as to attract the attention of all the operators on the line. It is not unusual to see a beginner sweating profusely in a cold room from the exertion of taking an easy ten-word message. A similar fact appears in the difficulty which the young operator has to keep up a conversation. He writes very slowly, and

yet he cannot think of enough things to say. The organizing imagination seems paralyzed by the presence of his audience. In one who is not an expert the emotions of fear, of anger and even of joy, generally paralyze invention, so that only spasmodic or meaningless sound groups can be made, and every one recognizes that the man is 'rattled.' The ability to receive is also often so affected that he is unable to recognize anything. This is particularly true in the case of fear.

The telegraphic language becomes so thoroughly assimilated that thinking apparently resolves itself into the telegraphic short hand used in conversation. This telegraphic short hand is an abbreviated code in which the vowels and many consonants are thrown out. One thinks in telegraphic terms. An odd expression or an unusual message attracts the operator's attention, while he is directly engaged with some other work. Operators who work at night depend on their office call to waken them. The sensation is that of hearing one's name repeated softly over and over. When the operator is worn out by loss of sleep or physical fatigue he is, of course, more difficult to awaken. At such times the sender writes the office call very distinctly and makes unusually long spaces. This seems to add emphasis to the call. The anger flutter, a whirl made by rapid alternate strokes of the first and second finger, is also employed with good effect to awaken sleeping telegraphers.

How thoroughly the telegraphic language is mastered in some cases is illustrated by the fact that expert operators 'copy behind' three or four words; sometimes ten or twenty words; that is, the receiving operator allows the sender to write a number of words before he begins to copy. It is then possible for him to get something of the sense of the sentence in advance. The operator is thus able, not only to punctuate and capitalize, but also to keep run of the grammatical structure. Yet, while he would detect an error, or notice that a word was not appropriate in the connection used, and be able to suggest to the sender what the word should be, the language of the message as a whole may have little or no meaning to him. Several cases illustrating this fact have been observed. The most notable case was given by Chief S. A message for the superintendent

was received by a very skillful operator. Any one might have seen from its contents that it required immediate delivery and action. Some word in the message had been so written that it was misread and missent by the sending operator. The receiving operator saw that this word did not make sense, and suggested to the sender that it might be another word similar in appearance. The sender decided that this was true and the message was corrected accordingly. The receiving operator placed the message upon the superintendent's hook, where it was found by a clerk too late for the action required. The receiver could not believe that he had received such a message until his copy was shown him. The details described were then recalled. The railway companies recognize this tendency to automatism by requiring dispatchers not to send out train orders until they have been assured by the operator to whom the orders are sent that he has displayed the proper signals.

The most striking example of complete mastery of the telegraphic language is seen in the daily work of a train dispatcher on a trunk line. Except when there is a very unusual amount of traffic, the dispatcher records the movements of trains as reported on a train sheet, figures on a special meeting point for trains, sends out the order, and as it is repeated by one office, copies it in the order book, checks it again as the next office repeats it, acknowledges its correctness and gives his official sanction; but while this very important work is being done, he figures on other meeting points in which the weather, length of side tracks, size and heaviness of trains, grades and probable delays are items. By the time the first order is completed, he has decided on the next line of action, and so it goes on for the eight hours he is on duty.

When not influenced by nervous diseases, practice enables nearly all to make groups of four, five or six dots with great rapidity. For the accuracy with which this is done see below part IV. Measurements made with the chronograph showed this rate to be as high as twelve dots per second. Letters forming words are written much more rapidly and with greater precision than letters taken at random. The same is true of words in connected discourse as compared with words in random order.

The effect of emotion upon sending is to give greater facility of expression to expert men, while beginners are in a greater or less degree paralyzed. The syllable 'ha' repeated, indicates laughter and is frequently used without any other external sign even when the operator's risibles are considerably excited. The uninitiated spectator might not suspect that the conversation was humorous. On the other hand, the anger flutter described above is invariably accompanied by a strong facial expression of passion.

Tests were made to ascertain the average rate of sending. The best results were obtained from two one-minute trials of dispatcher K. At the first trial he wrote thirty-nine words of a hundred and eighty-six letters, making four hundred and sixteen impacts upon the key. At the second trial he wrote forty-two words of a hundred and ninety-two letters, or four hundred and twenty-nine impacts. This shows a speed of seven and two-tenths movements per second. If the words in the Cincinnati contest, where the winner wrote forty-nine words per minute, averaged as many impacts per word as in the above cases, the rate would be eight and one-tenth impacts per second. Comparison of these results with the maximum rate of voluntary movement as determined by Von Kries,¹ Dressler² and Bryan³ must, of course, take into account the important differences between the conditions in the tests made.

Every operator develops a distinctive style of sending so that he can be recognized readily by those who work with him constantly. (See III. below.) Mr. S., a dispatcher of much experience, works daily with forty or fifty men and states that, after hearing four or five words, he can readily recognize the sender, or be sure that he is not one of his men. Where two or more operators work in the same office they sometimes change before the appointed time, or work for each other without permission. When a train is reported, however, in such cases, the dispatcher often asks where the other operator is. Operators who feel secure in the seclusion of their offices have

¹ Du Bois Reymond, *Archiv f. Physiologie*, 1886. Suppl. I.

² *Am. Jour. Psychol.*, IV., p. 514.

³ *Am. Jour. Psychol.*, V., p. 1.

sometimes been detected in making improper and impertinent remarks on the line, by their style of sending. Two instances were noted where men were discharged for offenses detected in this manner. Young operators have a peculiar way of grouping the letters of words, which gives the impression of some one walking unsteadily as when partially intoxicated. Many dispatchers claim that they can generally recognize a woman by her style of sending.

The best time to learn telegraphy is doubtless before the age of eighteen. The most expert operators have learned as a rule, when quite young. It is very difficult and often impossible to become even a passable operator, when the start is made after thirty. While extreme age weakens and limits the power of the operator, the maximum skill seems to be retained up to the age of sixty-five. This point, however, demands special investigation. Severe headaches and other painful diseases interfere with the work of the operator. Mr. S., now a train master, regards the use of tobacco as hurtful to operators. Mr. W., a Western Union Superintendent thinks tobacco invariably injurious to his men. Many operators do not consider a moderate use of tobacco as detrimental. All agree, however, that intoxicants make a man not only unreliable, but dangerous. To be found in a saloon means discharge on many railroads. The work of telegraphers is much affected by nervous diseases. Writers' cramp frequently disables the sending arm, and causes the retirement of the operator, unless he learns to send with the other arm, a difficult matter with most men so afflicted. Sometimes rest and treatment relieve this difficulty, at least temporarily. Mr. Y. had suffered from a nervous affection which made it difficult for him to stop when making four or five successive dots. He would make ten or twelve dots in writing letters composed of these groups.

III. INDIVIDUAL DIFFERENCES IN TELEGRAPHIC WRITING.

The telegraphic language is singularly well adapted to the experimental study of many problems in physiology, psychology and even philology. Indeed, if one were required to invent a generation-long experiment for the exact study of certain phases

of language, one could scarcely hope to find a better; for, on the one hand, no other language used by man can be so completely translated into exactly measurable symbols; while, on the other hand, the manifold personal differences in the operators are shown by investigation to be represented in those symbols. As illustration and proof of these assertions, the following study of individual differences in telegraphic writing is offered.

Apparatus.—The DuPrez signal was adjusted to write upon the Marey drum, being carried transversely by the automatic carriage which Verdin supplies with the Marey drum. The Marey chronograph in circuit with the Kroneker Interrupter gave the time control.

The experiment.—The sentence, "Ship 364 wagons via Erie quick," was written by each subject about a dozen times in succession. This sentence was chosen because it contains almost every sort of difficulty which the telegraphic language presents. 'Ship' is composed of groups of three, four, two and five dots. Each of the figures 3, 6, 4 is somewhat difficult, especially the 6, which is composed of six dots. 'Wagons' contains two letters which have two dashes in succession. 'Via' is a simple word presenting no special difficulty. 'Erie' is by far the hardest word in the sentence. By a little change in the time relations, one would get oye, erc, sic, eeye. Such words are usually written with extra care and with longer spaces. The word 'quick' has several dashes, of which the last one is likely to be longer than the others, since it closes the word and the sentence. The sentence as a whole is as follows: S (...), h (....), i (..) p (.....), 3 (...—.), 6 (.....), 4 (....—), w (.—), a (.—), g (—.), o (..), n (—.), s (...), v (...—), i (..), a (.—), E (.), r (..), i (..), e (.), q (..—), u (—), i (..), c (..), k (—.).

The subjects.—By connecting the Du Prez signal with the main lines of the Western Union Telegraph Company and of the Monon Railroad Company, about sixty operators were tested. It was found that it would require several years of continuous work to investigate all of these records by the method decided

upon. Accordingly sixteen of the number were selected, some of them being expert, some ordinary and some poor operators. Following is a brief characterization of the men, grouped somewhat in the order of their ability as telegraphers, beginning with the less expert. A. B. Guthrie, student I. U., age 21, 5 months' experience as student in a telegraph office; C. G. Mallotte, student I. U., age 22, 2 years' experience as student in an office, 3 months in charge of a small office; Prof. G. E. Fellows, department of European history, I. U., age 43, 5 years' experience in charge of railroad office about 13 years ago; R. C. Brooks, student I. U., age 21, 3 years' experience in charge of an office; L. A. Clark, agent Monon, Crawfordsville, Ind., age 40, experience 17 years; C. L. Buchanan, agent Monon, Ellettsville, age 57, experience 25 years; G. W. Dyer, agent Monon, Bainbridge, Ind., age 60, experience 33 years; Geo. H. Godfrey, Manager W. U., New Albany, Ind., age 55, experience 31 years; Mrs. Z. M. Apple, Manager W. U., French Lick Springs, Ind., age 24, experience 5 years; Miss Nellie Green, operator Monon, Louisville, Ky., age 24, experience 5 years; A. B. Evans, Manager W. U., Bloomington, Ind., age 27, experience 10 years; Noble Harter, graduate student of Psychology, Indiana University, age 37, experience 21 years; C. W. Goodman, dispatcher, Monon, age 32, experience 15 years; H. O. Chapman, dispatcher, Monon, age 27, experience 10 years; W. H. Fogg, dispatcher, Monon, age 26, experience 10 years; E. B. Cassell, chief dispatcher, Monon, age 36, experience 18 years.

Measurement of Results.—Each character in the tracings obtained was measured. There are one hundred and forty-nine characters to be measured if the sentence is written without error; but, as in many cases, too many characters were made, the actual number to be measured was considerably greater. Eight repetitions of the message by each of the sixteen subjects were measured, so that the total number of measurements made was about twenty thousand, and required several months. The measurements were made to the nearest half millimetre. The rate of the drum was so adjusted that forty-seven mm. correspond to one second of time. Most of the errors must then evi-

dently be less than five thousandths of a second, and an error of $\frac{1}{2}$ mm. would be about one hundredth of a second. Higher accuracy could have been obtained easily, and was obtained in individual cases by increasing the rate of the drum and so lengthening the lines to be measured. But upon careful consideration, the degrees of accuracy given was proved to be sufficient for the experiment proposed.

Methods of Treating Results.—Several methods of treating the results were employed for the purpose of bringing out different points.

First Treatment.—The theoretical Morse alphabet is composed of the following elements: A, the dot, one unit of time; B, the dash, three units of time; C, the short space between the parts of a letter, one unit of time; D, the long space, in spaced letter, two units of time; E, the space between letters, three units of time; F, the space between words, six units of time. This is the ideal scheme which each operator has tried to learn. Now it is possible to compare the actual writing of each individual with this ideal scheme by taking the actual length of his dot as a unit and by then computing the actual ratios of the other elements to this unit. This computation was made for each individual. The average length of dot, dash, etc., being used. The results are given in Table I.

It was found by a study of the individual results that the several values of a given character are not accidental variations from their average value, but that there are constant differences between the times required for the same character in different parts of the sentence or even of the same word. These facts, which affect the value of Table I., will be considered further on. The table proves its value in spite of this or any other defect, however, by the fact that it affords a means of identifying any one of the individuals represented in it. If a single one of the sentences written by one of the subjects be treated by the method employed in constructing the table, a comparison of the several ratios obtained and their sum with the values given in Table I., will in every case show a correspondence so much more exact with one of the sixteen than with any of the others, that the identification will be beyond question.

TABLE I.

NAME.	A	B	C	D	E	F	TOTAL.
IDEAL CODE	1.00	3.00	1.00	2.00	3.00	6.00	16.00
C. G. MALLOTTE	1.00	1.95	1.13	1.55	1.76	2.09	9.48
A. B. GUTHRIE	1.00	4.66	2.33	3.51	5.80	7.44	24.74
R. C. BROOKS	1.00	3.02	1.18	2.41	2.88	4.05	14.54
G. E. FELLOWS	1.00	3.47	1.24	3.09	5.32	6.76	20.88
A. B. EVANS	1.00	2.30	.89	1.98	2.10	2.65	10.92
NOBLE HARTER	1.00	2.83	.95	2.31	3.08	6.44	16.61 ✓
G. W. DYER	1.00	2.85	1.09	2.20	2.93	5.37	15.44
G. H. GODFREY	1.00	2.27	.76	1.77	2.02	4.15	11.97
C. L. BUCHANAN	1.00	2.64	1.02	1.86	2.50	4.68	13.70
L. A. CLARK	1.00	2.38	.70	2.31	2.95	4.91	14.25
Z. M. APPLE	1.00	2.45	.94	1.77	2.45	3.58	12.19
NELLIE GREEN	1.00	2.49	.85	1.81	1.92	2.87	10.94
W. H. FOGG	1.00	2.98	1.08	2.40	2.71	3.00	13.17
E. B. CASSELL	1.00	2.61	1.06	2.23	3.01	4.12	13.92
C. W. GOODMAN	1.00	2.32	.87	2.13	2.42	3.14	11.88
H. O. CHAPMAN	1.00	2.50	.94	1.97	2.87	3.36	12.71

In order to obtain average values which should represent more homogeneous values and also to show characteristic individual differences in a single short and easy word, the results for the word 'via' were treated in the following way. As above, each man's average dot for the six dots in the word was taken as the unit, and the length of each character was computed as a per cent. of that unit. The ideal scale is given for comparison, also, the variation of this average dot from the typical dot, *i. e.*, the average of all the dots in the sentence.

In order to prove the identifying value of this table, two tests were made: A friend selected single records from three of the operators. Computations of these records by the same method as that used in making the table gave results which could be unmistakably identified. Then, records of three others were measured from the original tracings and the results

TABLE II.

NAME.	A.	C	A.	C	A.	C	E	A.	C	A.	E	A.	C	B	F	Total.	Dot or Unit.	Typi- cal Dot.	Var.	
C. G. MALLOTTE	1.07	1.11	.93	1.22	1.22	1.22	1.47	.93	1.00	.93	1.97	.89	1.07	2.29	2.36	22.19	2.79	3.05	.26	
A. B. GUTHRIE .	1.19	2.10	1.10	1.95	.71	2.24	4.86	.81	1.76	2.24	4.67	.95	1.86	4.33	6.14	40.91	2.10	1.92	.18	
R. C. BROOKS . .	.85	1.32	1.06	1.28	1.32	1.36	2.60	.85	1.19	1.06	3.67	.85	1.02	4.04	5.06	29.06	2.35	2.45	.10	
G. E. FELLOWS .	1.07	1.23	1.00	1.29	.89	.87	3.99	1.07	1.26	1.03	5.16	.96	1.12	3.52	6.49	37.29	3.02	2.97	.05	
A. B. EVANS	.95	.86	.90	.72	.90	.99	2.52	1.44	1.12	1.04	2.25	.90	.95	3.11	2.93	22.80	2.22	2.39	.17	
NOBLE HARTER	.76	1.03	1.21	1.03	1.35	1.10	3.11	.83	1.03	1.10	2.48	.76	1.00	3.42	6.69	29.53	2.90	3.12	.22	
G. W. DYER . .	.91	1.16	1.19	1.03	1.23	1.03	2.81	.91	1.30	1.04	2.80	.68	1.01	3.43	6.54	29.15	3.07	3.28	.21	
G. H. GODFREY	.92	.95	.95	.95	1.10	.83	2.38	.89	.86	1.19	2.09	.92	.74	2.69	4.09	22.65	3.35	3.72	.37	
C. L. BUCHANAN	.96	.99	1.21	1.02	1.11	1.21	2.82	.96	1.05	1.08	1.48	.59	1.11	4.37	4.31	25.35	3.23	3.44	.21	
L. A. CLARK . .	.91	.59	1.13	1.06	1.00	.85	2.70	.94	.75	1.03	2.22	.97	.68	3.82	7.21	27.77	3.19	3.43	.24	
Z. M. APPLE . .	1.00	.86	1.04	1.07	1.10	1.21	2.94	1.38	.93	.76	2.01	.86	.93	3.18	3.25	23.56	2.89	3.06	.17	
NELLIE GREEN .	1.03	.81	1.03	.96	.92	.96	2.57	.96	.85	.88	.99	2.12	.81	.85	2.94	4.08	22.76	2.72	2.69	.03
W. H. FOGG . .	.66	1.13	1.17	1.13	1.17	1.17	2.72	1.83	.89	1.17	2.22	.78	1.05	3.66	3.15	25.22	2.57	2.69	.12	
E. B. CASSELL .	.92	1.10	1.10	1.03	.96	1.07	2.54	2.35	.88	1.07	2.09	1.03	.92	3.31	5.22	26.69	2.72	2.76	.04	
C. W. GOODMAN	.92	.62	1.10	.92	1.03	.92	2.46	1.69	.82	1.10	2.28	1.03	.88	2.64	3.71	23.22	2.72	2.78	.06	
H. O. CHAPMAN	1.05	.98	1.12	1.05	1.22	.98	2.44	1.74	.77	.94	2.44	.80	.84	2.86	4.18	24.46	2.87	3.02	.15	
IDEAL	1.00	1.00	1.00	1.00	1.00	1.00	3.00	1.00	1.00	1.00	3.00	1.00	1.00	3.00	6.00	29.00	2.80	2.92	.12	
																	Av.	Av.		

TABLE III.

No.	A .	C	A .	C	A .	C	B .	E	A .	C	A .	E	A .	C	A .	F	T't'L.	AV. DOT.	IDENTIFIED AS
1	(a)	.51	1.15	1.15	1.15	1.15	2.70	1.74	.97	1.15	1.35	2.30	.77	.97	3.67	3.09	24.97	2.59	W. H. F.
	(b)	.66	1.13	1.17	1.13	1.17	2.72	1.83	.89	1.17	1.32	2.22	.78	1.05	3.66	3.15	25.22	2.57	
2	(a)	.93	.93	1.23	1.08	1.08	2.82	.93	1.08	.93	1.08	1.48	.61	1.08	4.61	4.31	25.37	3.25	C. L. B.
	(b)	.96	.99	1.21	1.02	1.11	2.82	.96	1.05	1.08	1.08	1.48	.59	1.11	4.37	4.31	25.35	3.23	
3	(a)	.99	.78	1.07	.99	.99	2.54	.99	.88	.99	.99	2.11	.78	.99	3.09	4.10	23.27	2.56	N. G.
	(b)	1.03	.81	1.03	.96	.92	2.57	.96	.85	.88	.99	2.12	.81	.85	2.94	4.08	22.76	2.72	
4	(a)	.92	1.12	1.18	1.12	1.28	2.61	2.04	.92	1.28	.92	2.61	.74	.92	3.33	5.42	27.53	2.67	G. W. D.
	(b)	.91	1.16	1.19	1.03	1.23	2.81	2.08	.91	1.30	1.04	2.80	.68	1.01	3.43	6.54	29.15	3.07	
5	(a)	.94	.94	.94	.94	1.10	2.36	1.10	.94	.84	1.10	2.05	.94	.74	2.66	3.95	22.33	3.17	A. H. G.
	(b)	.92	.95	.95	.95	1.10	2.38	1.10	.89	.86	1.19	2.09	.92	.74	2.69	4.09	22.65	3.35	
6	(a)	1.00	.75	1.00	.75	.75	2.50	1.25	1.00	1.00	1.25	2.25	1.00	1.00	3.00	3.00	22.50	2.00	A. B. E.
	(b)	.95	.86	.90	.72	.90	2.52	1.44	1.12	1.04	1.22	2.25	.90	.95	3.11	2.93	22.80	2.22	

(a). The unknown record given in terms of mm. reduced to per cents. of the average of the six dots contained in it, as in Table II.

(b). A row of per cents. from Table II, selected as representing the unknown sender, because of its greater resemblance to (a). The letters A, B, C, etc., refer to the several Morse characters.

computed as above. The identification was equally sure. Table III. gives the six sets of results obtained in this way in comparison with the corresponding results from Table II. Of special importance is the fact that the individual results, numbers four, five and six, Table III., were written much more swiftly than the results represented in Table II. by the same operators; for it is thus shown that the individual characteristics persist through different rates of speed. The variation in speed does not, however, leave the ratios between the several characters undisturbed. Examination of Table III. will show that greater speed is gained for the most part by shortening the longer characters. Fig. 1 in Plate I. shows the relations of Table III. graphically.

Third treatment.—By a different method, a single letter *v*

TABLE IV.

OPERATOR.		A	C	A	C	A	C	B	E	TOTAL.
H. O. C.	(a)	.099	.093	.105	.099	.116	.092	.231	.165	1.000
	(b)	.098	.094	.105	.098	.116	.094	.230	.165	1.000
G. H. G.	(a)	.101	.101	.105	.105	.121	.091	.258	.118	1.000
	(b)	.097	.097	.104	.106	.119	.091	.258	.128	1.000
G. E. F.	(a)	.063	.073	.059	.076	.055	.054	.236	.384	1.000
	(b)	.067	.067	.058	.081	.058	.058	.230	.381	1.000
A. B. G.	(a)	.067	.114	.060	.109	.042	.126	.265	.217	1.000
	(b)	.069	.118	.063	.105	.043	.128	.260	.24	1.000
N. G.	(a)	.111	.093	.111	.102	.100	.105	.275	.103	1.000
	(b)	.114	.089	.112	.101	.100	.102	.274	.108	1.000
N. H.	(a)	.065	.089	.100	.089	.109	.090	.248	.210	1.000
	(b)	.063	.090	.100	.090	.110	.088	.250	.209	1.000

(a) The ratios of the average length of each element, to the average length of the whole letter *v*.

(b) One of the eight percentages selected at random, the design being to demonstrate its efficiency in identification.

from the word 'via' was studied. The average length of each element was found, and the ratio of this to the average length of the whole letter was computed. Each of the eight trials for six of the subjects chosen at random were thus treated. The results are given in Table IV., and they show that each of the six may be identified by the characteristics which appear in the single letter.

Variation:—Sixty-nine dots are required to write the message used. An operator wrote the message eight times. It is evident that the sixty-nine dots made in one writing of the message will vary, and also that each of the sixty-nine dots will vary in the eight successive writings thereof. To save circumlocutions, I shall call the first *heterotaxic* variation, and the second *homotaxic* variation. These two sorts of variation were studied in a variety of ways, covering all the material at disposal, and always with the same result. In order to make a sure, quantitative comparison between the two sorts of variation, it was decided to study eight dots, beginning with the first dot in the word 'via.' Since the message was written eight times, this selection gave eight rows and eight columns, or sixty-eight dots in all, for each of the sixteen subjects. The homotaxic variation was computed as follows: The average, the mean variation ($\frac{\sum v}{n}$), and the per cent. which the latter is of the former were computed for each of the eight columns. The average of these eight results is a measure of the homotaxic variation. The heterotaxic variation was found by two methods. First, the rows were treated in the same manner as that used in finding the homotaxic variation in the columns. Second, the average of each column was found, and then the per cent. of variation in these averages was computed. The reason for this procedure will be given below. Table V. shows the results obtained.

It is evident from the foregoing table that the homotaxic variation is an inverse measure of skill. In every day language, this simply means that an operator can repeat the same action more exactly the more expert he is.

The heterotaxic variation as given in column II. is a resultant of two factors. First, of course, the accidental variation

TABLE V.

OPERATOR.	AGE.	YEARS EXPERIENCE.	HOMO- TAXIC I.	HETERO- TAXIC II.	HETERO- TAXIC III.
C. G. MALLOTTE	22	2	.117	.120	.086
A. B. GUTHRIE	21	5-12	.112	.161	.150
G. E. FELLOWS	43	¹ 5	.094	.097	.035
G. W. DYER	60	33	.078	.154	.150
NELLIE GREEN	24	5	.075	.156	.150
C. L. BUCHANAN	57	25	.070	.152	.130
L. A. CLARK	40	17	.062	.078 ¹	.053
NOBLE HARTER	37	21	.056	.171	.170
R. C. BROOKS	21	3	.052	.135	.120
Z. M. APPLE	24	5	.050	.090	.072
A. B. EVANS	27	10	.047	.170	.170
C. W. GOODMAN	32	15	.045	.104	.088
H. O. CHAPMAN	27	10	.045	.116	.083
G. H. GODFREY	55	31	.041	.120	.106
W. H. FOGG	26	10	.034	.192	.180
E. B. CASSELL	36	18	.027	.092	.075

enters here as everywhere. Second, the operator *does not intend* to make the same character exactly alike in successive positions. This intentional differentiation corresponds to inflection in speech. A study of Table II. or III. will show clearly these characteristic differences between a character in one position and the same character in a different position. By computing the heterotaxic variation by the second method described, we partially eliminate the element of accidental variation. A comparison of the variations given in columns II. and III. of Table V. shows that the amount of accidental variation thus eliminated is, as might be expected, smaller in the case of the expert men. If we take the results in column III., Table V. as the best attainable measure of variation due to intentional inflection, it appears that this is slightly greater in the more expert men than in the less expert. The average heterotaxic

¹ Has been out of business about 18 years.

variation of the first seven, or the less expert operators, as determined from column III. of Table V. is 106. The corresponding value for the last nine, the more expert men, is 118. The largest two values in the Table belong to two of the more expert men, and the smallest two belong to two of the less expert men.

In conclusion, therefore, it may be said that the accidental variation is a somewhat accurate inverse measure of skill, while the variation for inflection, is likely to be larger rather than smaller with increasing expertness.

Note on the accuracy with which the longer groups of clicks are made by telegraphic operators. A count of the results show in writing.

h (....) 5 errors out of possible 128.

v (...—) 10 “ “ “ “ “

q (..—.) 4 “ “ “ “ “

3 (...—.) 12 “ “ “ “ “

4 (...—) 17 “ “ “ “ “

p (.....) 56 “ “ “ “ “

6 (.....) 91 “ “ “ “ “

In order to show the variations in writing the figure 6 (.....), and that these variations are not primarily determined by the skill of the operator, the results for the several operators are given.

V. THE CURVES OF IMPROVEMENT IN RECEIVING AND SENDING.

Throughout the year of exploration, operators were questioned closely with regard to the rate of improvement with practice at various periods. Operators generally agreed upon certain main facts. Upon the basis of this general inquiry and of his own personal experience as an operator and a teacher of telegraphy, H. drew the curves represented in Fig. II., Plate I. as a rough picture of the facts.

In further verification of the main characteristics of these curves over two hundred operators, ranging in skill from the

OPERATORS.	YEARS EXPERIENCE.	PRESENT POSITION.	AV. NO. OF DOTS IN WRITING 6.
C G M	2	Student	5.2
A B G	5-12	"	6.0
G E F	5	College Professor	5.6
G W D	33	Ry. Agent	9.2
N G	5	Ry. Operator	10.2
C L B	25	Ry. Agent	8.6
L A C	17	" "	6.1
N H	21	Teacher	7.5
R C B	3	Student	7.1
Z M A	5	W. U. Operator	6.0
A B E	10	" "	7.2
C W G	15	Dispatcher	6.1
H O C	10	"	7.7
G H G	31	Ry. Agent	12.2
W H F	10	Dispatcher	5.0
R B C	18	"	8.7

most expert to those just beginning, have been questioned and have given practically unanimous assent.¹

For the purpose of gaining not only more, but more definite information, requests were sent to a number of schools of telegraphy. Blanks were sent to such schools with the request that the typical curves of improvement be plotted. In many cases, of course, no reply was received, and in some cases the advertising impulse was the evident inspiration of curves which represented their students as becoming skillful operators in a few months. The manager of Valentine's School of Telegraphy, Jonesville, Wis., wrote: "While I recognize the fact that every student undergoes the experience you have outlined in your letter, I hesitate to furnish you this information without first having systematically obtained it." Seven months later he sent the

¹ One skillful operator denied having experienced the period of non-advancement shown in the receiving curve before reaching the main-line rate. His associates in the office explained this exception as a lapse of memory due to vanity. Their incredulity illustrates the practical unanimity of opinion.

curves in Fig. III., Plate I., which are believed to be entirely reliable, as is also Fig. IV., Plate I. from the Commercial College of Kentucky.

Two schools where telegraphy is taught in Cincinnati were visited personally and arrangements made to have the progress of a representative student in each observed systematically. These results are given in Figs. V. and VI., Plate I. Arrangements were also made to have two reputable operators, well known to H., observe and test the progress of one student in each of their offices, from the time of beginning until proficiency was reached. These results are given in Figs. VII. and VIII., Plate I.

Finally H. was able, during the winter 95-96, to test the advancement of two learners from the beginning until they were both fair operators. Both were students in the Western Union Office at Brookville, Indiana. The operator, Mr. Balsley gave every assistance in his power to make the investigation successful. Will J. Reynolds, one of the students, is eighteen years old and is a young man of more than ordinary ability. Edyth L. Balsley, the other student, is seventeen years old and is a very bright young girl. The former began in August, the latter in September, 1895. The tests were made every Saturday. Forty tests were made with the young man and thirty-six with the young woman.

Ordinarily telegraphic speed is reckoned in terms of so many words per minute. For these tests, however, the letters were counted. Of course sentences were used in each test which had not been used before. Pains were taken to keep the tests of uniform difficulty. On the one hand, many short and easy combinations, and on the other hand, combinations representing unusual difficulty from a telegraphic point of view were avoided. Special pains were also taken to see that the amount of practice from week to week was substantially uniform.

The sending test was made as follows: The learner was directed to write as fast as he could do so, legibly. The observer copied the words as sent as a test of legibility. Some two-minute period was noted by the observer, unknown to learner, and the number of letters sent in that time was afterward

counted. Several tests were taken and the results averaged. The variation in the several tests was slight¹. The receiving test was made as follows: The observer would try a rate of sending which he judged would correspond to the learner's capacity. The learner was required to name the letters, later on the words, or, when he had more skill, to copy without naming them. If he failed to interpret correctly at that rate, a slower rate was tried. If he succeeded, a more rapid rate was tried. A two-minute period was noted and the letters were counted as above.

The results of this study are shown in Figs. IX. and X.²

Significance of the practice curves. Certain main facts appear in all the foregoing curves:

1. The sending curve rises more rapidly and more uniformly than does the receiving curve from the beginning of practice to the learner's maximum ability.

2. The receiving curve rises more slowly and irregularly. All the results agree in showing a long, flat curve for several months before the slowest main-line rate is reached; and all the evidence before us indicates another long flat curve a little above the rate necessary for the transaction of ordinary office business, in the case of operators to whom that amount of skill in receiving is sufficient. A study of the quantitative results shown in Figs. IX. and X. shows that there are many short flat places in the receiving curve followed by relatively rapid improvement.

3. Two of the curves show a fact which usually appears at a period of the learner's development later than that shown in these curves, namely, that the receiving rate finally exceeds the sending rate. This is almost the universal rule. A receiving operator with a typewriter can practically take his ease in taking the most rapid press work.

¹ The M. V. ranged from .37% to 2.3% of the averages.

² It is believed that the progress of the learners was materially hastened by their interest in the tests. They were forewarned as to the slowness of progress and they gave special attention to practice. Both are now (June 1896) able to transact ordinary business on the main line. It may prove to be worth while for certain purposes to study the curves of improvement with more accurate methods and apparatus, but there can be no doubt that the method used gives a highly accurate quantitative picture of these curves.

Fig. IX.

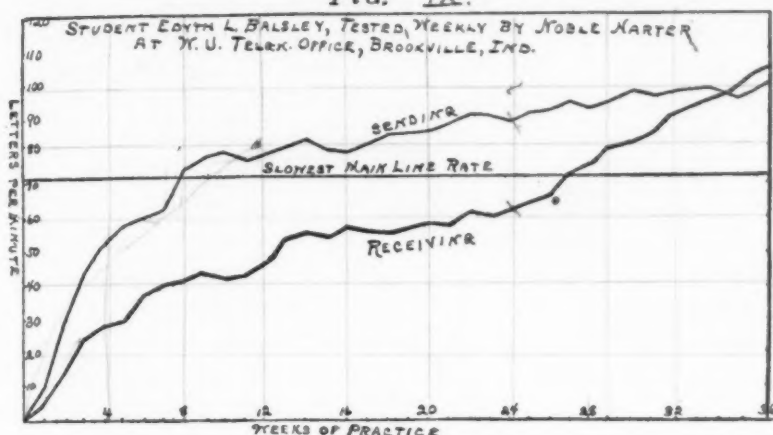
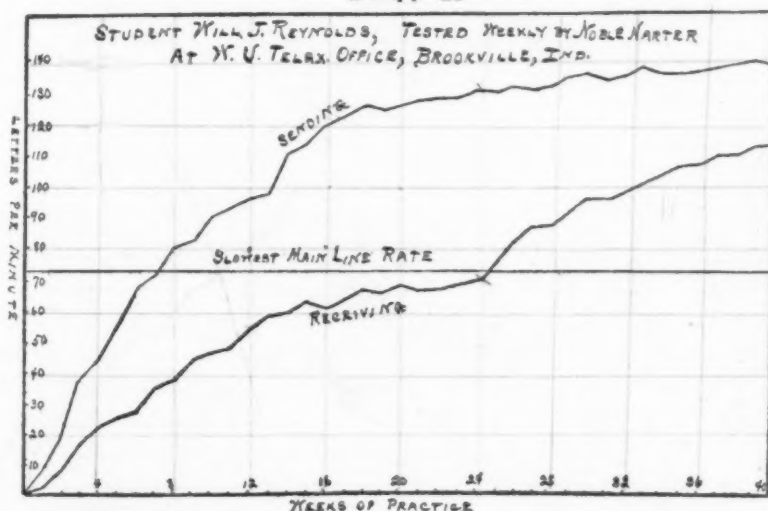


Fig. X.



4. In considering the reasons for the remarkable differences between the receiving and sending curves, the following points may be noticed: (a) The language which comes to the ear of the learner seems to him far more complex than the language which he has to write. When he wishes to write the letter *e*, he must have in mind only the making of one quick snap with his hand. When he *hears* the letter *e*, he hears two sounds,

the down stroke and the back stroke, and must take note of the time between them to distinguish the dot from the dash. If we take the more difficult combinations, as *k* (— . —), or *j* (— . — .), the greater complexity of the sound picture with its irregularly occurring back stroke is sufficiently evident. (b) The opportunity for practicing receiving at slow rates is evidently far less than for sending at such rates. It is always possible for the learner to do his slow best at sending, but he must depend upon others for a chance to receive at a rate within his capacity. It is of course true that he hears all that he himself sends, but it is a significant fact that the hearing of his own writing does not improve his power to receive in anything like the same degree that the hearing of other operators' writing does. As the curves show, young operators can, at a certain period, send with fair rapidity for a long period during which they cannot understand a single sentence on the main line. (c) A further significant fact is that learners enjoy the practice of sending, but feel practice in receiving to be painful and fatiguing drudgery. For this reason they naturally incline to practice sending a great deal, but must summon up all their resolutions to keep up the necessary practice in receiving. (d) A fact which seems to be highly significant is that years of daily practice in receiving at ordinary rates will not bring a man to his own maximum ability to receive. The proof of this fact is that men whose receiving curve has been upon a level for years frequently rise to a far higher rate when forced to do so in order to secure and hold a position requiring the higher skill. That daily practice in receiving will not assure improvement is further seen in the fact that in many cases inferior operators after being tolerated for years are finally dropped because they do not get far enough above the dead line. (e) One conclusion seems to stand out from all these facts more clearly than anything else, namely, that in learning to interpret the telegraphic language, it is intense effort which educates. This seems to be true throughout the whole length of the curve. Every step in advance seems to cost as much as the former. Indeed, each new step seems to cost more than the former. Inquiry at the telegraph schools and among operators indicates that between sixty

and seventy-five per cent. of those who begin the study of telegraphy become discouraged upon the plateau of the curve just below the main-line rate. As a rule, ordinary operators will not make the painful effort necessary to become experts. Facts of an analogous character will be recalled from other fields.

The physiological, psychological and pedagogical implications of this conclusion are manifestly important. If in our educational methods in the past, we have often made the pace that kills, there is possibly the danger on the other hand that we shall make school work all play, and so eliminate the intense effort which is necessary for progress.¹

5. The sending curve conforms approximately to the well-known typical practice curve with the important difference from the curves usually obtained in the laboratory that it extends over a much greater period of time. This difference characterizes the whole curve. If we represent the practice curve by the general equation

$$y = f(x)$$

it is evident that the function of x contains a constant which depends upon the unit of time. So for example, the curve given in the figure would present exactly the same appearance if the same results had been obtained in forty successive hours or forty successive years. Comparison of different practice curves shows that this time factor varies greatly in the development of different abilities. A comparative study of this charac-

¹The conclusion here reached in matters of learning telegraphy recalls the opinion of Senator Stanford upon the training of race horses. In a letter to Horace Busbey (*Scribner's*, June, '96), Senator Stanford says:

"My own idea, and I think it is justified by experience, is to commence working the colt early, developing its strength with its growth. If the exercise is judicious, the colt takes no harm from it. I do not remember a single instance where an animal of mine was injured by early work. When a breakdown has occurred, it has been invariably after a let-up. Let-ups are very dangerous to young fast animals, as their bodies grow during the let-up without corresponding development of strength, and they are very liable to get too much work when their exercise is renewed. My aim is to give the greatest amount of exercise without fatigue, and never to allow it to reach the period of exhaustion. This is secured by short-distance exercise. It is the supreme effort that develops. If colts are never over-worked they are always willing to try in their exercise, having no apprehension that they will be forced beyond their comfort."

teristic of various practice curves would have evident theoretical and practical values.

6. The receiving curve presents many profound interests and difficulties. It is a quantitative study of apperception. It represents with a high degree of accuracy the increasing power which practice brings to interpret a language. The task of the mind is not in every respect analogous to that involved in listening to foreign speech, for in the telegraphic language, after a short time, every element is recognized by the learner if he is given time, whereas in the foreign speech he may frequently be disturbed by words that are entirely unknown. We have, however, gained the impression, partly from personal experience and partly from conversation with teachers of language and others who have learned foreign languages, that the curve of practice in learning a language must present at least great general similarities to the receiving curves here shown. All agree that just below the ability to understand what is spoken, there is a long discouraging plateau where many give up in despair; that there is at last a sudden ascent into the ability to understand most of what is said; finally that the perfect mastery of one at home in the language, comes much later and only after very persistent work. Of those who undertake the study of any foreign language, most stop on the first plateau below a working proficiency; and of those who go on, most stop on the second plateau, below complete mastery.¹

7. What is the interpretation of the plateaus in the receiving curve? For many weeks there is an improvement which the student can feel sure of and which is proved by objective tests. Then follows a long period when the student can feel no improvement, and when objective tests show little or none. At the last end of the plateau the messages on the main line are, according to the unanimous testimony of all who have experience in the matter, a senseless clatter to the student—practically as unintelligible as the same messages were months before.

¹The President of the Cincinnati Business College, who prepared estimate VI., says that the same general characteristics appear in the learning of shorthand. A director of primary work in a western city, after examining the receiving curve, expresses the opinion that it represents the progress of a child in learning to read. This point will be made a subject of immediate investigation.

Suddenly, within a few days, the change comes, and the senseless clatter becomes intelligible speech. ✓

In explanation of the form of the *motor* curve, one may suppose that it is an asymptotic approach to a physiological limit. In the receiving curve there is also something like an asymptotic approach to a limit; but that limit, whatever its nature, suddenly disappears. What it is that occurs during the period of sensible and measurable improvement, may be represented in various ways according to the standpoint from which the mind and mental growth are viewed. But in every case, one has to account for the great slowing down in the process of improvement. Stated otherwise, the task is to explain the nature of the changes in brain or mind which must be taking place, during the period represented by the plateau, and which yet make no determinable manifestation of themselves. That changes are taking place during this period, which are essential to the acquisition of proficiency, is proved by the fact that no one is able to omit this period. ✓

8. As suggested above, it is probable that the curves which represent the acquisition of the telegraphic language also represent the main characteristics of the curves for many other acquisitions. This should, however, by no means be taken for granted. We should rather require of ourselves a thorough study of the actual curves of improvement for every ability which makes measurable manifestation of itself. The determination and comparative study of these curves would furnish one thing which is always a precious enlargement to any science—an outlay of problems which permit approach by systematic research and which promise results of far-reaching, theoretical and practical importance.

THE INFLUENCE OF INTELLECTUAL WORK ON THE BLOOD-PRESSURE IN MAN.¹

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Paris.

I.

Physiologists measure the blood-pressure in animals by applying the monometer to an artery; the height to which the column of the monometer is raised by the blood furnishes the measure of the pressure. A measure of the pressure in man can only be made from the exterior, the methods employed by Vierordt,² Marey,³ Kries, v. Basch, Mosso,⁴ Bloch, etc., all consist in suppressing the pulse, or in arresting the circulation in an organ that is easily accessible, *e. g.*, the hand and fingers, and in measuring the minimal pressure necessary for this suppression.

It would evidently be of the greatest value to combine these pressure-results with those which are furnished by the form of the arterial pulse, and by changes of volume in the limbs, in order to determine precisely the influence of intellectual work and of the emotions on the circulation. The question, *e. g.*, whether in any case an active or passive vascular dilation occurs, can only be answered with certitude by the monometer.

We undertook this psychological study in the belief that no one, up to this time, has reached satisfactory results. There are, it is true, some scattered observations in medical journals, on the pressure of the blood during intellectual labor, but the pressure has been generally taken with defective apparatus. The only systematic work on the subject, at least to our knowl-

¹ Translated for the REVIEW from the author's manuscript.

² *Die Lehre von Arterienpuls.* Braunschweig, 1855.

³ *Travaux du laboratoire*, 1876; p. 316.

⁴ *Arch. ital. de Biologie*, 1895; p. 177. This work contains a short historical sketch, a discussion of the researches of Basch, and the description of a new apparatus.

edge, is that of Kiesow; we will indicate later on what criticisms should be made on his work. In short, the problem which we have set before ourselves has no history.

Our first care should be the choice of a good instrument to measure the blood-pressure in man. Clinicists frequently employ the sphygmometer of Bloch, more or less modified by Verdin and Chéron; this instrument resembles Cattell's algometer. It is a pressure-dynamometer which obstructs the pulsation of the radial artery; the experimenter interposes his finger between the artery of the subject and the instrument, and receives on his own finger the pressure of the instrument in such a way that with the finger he presses upon and obstructs the artery of the subject, and at the same time perceives the pulsation of the artery; the method consists in gradually increasing the pressure of the exploring finger until it no longer perceives the beating of the artery which it compresses. We did not make use of this instrument; for after having tried it for some time, we rejected it because it involved a subjective element of estimation. We gave the preference to Mosso's sphygmomanometer which has the advantage of indicating its results by tracings.

It is unnecessary to give a description of the numerous parts which make up the apparatus; this description may be found in the work of Mosso and also in the *Année Psychologique*.¹ We will content ourselves with indicating the principle. Two fingers of each hand are placed in rubber finger-tubes, and through these tubes are exposed to the pressure of water; this pressure is varied by means of a piston and measured by a mercury manometer, which registers at the same time the pulsations of the fingers. If we begin with 0 and increase gradually, we find a regular change in the amplitude of the pulsation; this is very small at first, grows and reaches a maximum, decreases again, and finally disappears; thus the amplitude does not vary directly or inversely with the pressure; there is a most favorable degree of pressure, equal on the average to 80 mm. of mercury, at which the pulse attains its maximum amplitude; a weaker or stronger pressure have

¹ *Année Psychologique*, II., p. 584.

alike the effect of diminishing the pulse. The question is how to measure the blood-pressure with an instrument of this sort.

Marey's opinion was that we must take, as measure of the blood-pressure, the counter-pressure necessary to obstruct and suppress the pulse. Mosso maintained, on the contrary, that it is the most favorable pressure, about 80 mm. of mercury, which is equal to the pressure in the arteries. This disputed point is of little importance. What interests us as psychologists is not the absolute value of the blood-pressure, but the change which it undergoes by reason of mental processes; in relating our experiences we will have to examine the criteria of both Marey and Mosso, and to determine which of the two answers best to the special end which we have in view.

Technique. It is necessary to give some practical details as to the manipulation of Mosso's sphygmomanometer. Two very different methods may be employed together, since they supplement each other. A. The first method consists in registering the pulse with variations of pressure from 0 to 100 or 120 mm. of mercury, either varying the pressure by sudden jumps, *e. g.*, from 0 to 10, from 10 to 20, from 20 to 30, etc., or by slow changes of pressure which are almost insensible. The latter is the method recommended by Mosso.¹ It is very useful when one wishes to compare the blood-pressure to different hours of the day; it is necessary then to make the piston of the apparatus move from the minimal to the maximal pressure, in order to determine the value of the most favorable pressure. The differences of blood-pressure at different hours of the day can in this case be expressed in figures, an expression which is evidently the aim of all scientific research.

Here a parenthesis. In measuring the pressure with Mosso's sphygmomanometer, we must not take account of the absolute amplitude of pulsation, but of the counter-pressure necessary to give the pulsation its maximal amplitude. This is not at all the same thing. Mosso gives an interesting example which will make this distinction clear.² One of his subjects,

¹We found it practicable to make the piston revolve automatically with a weight-motor.

²*Op. cit.*, p. 180 f.

Dr. Colombo, has his blood pressure taken in the normal state; the most favorable point of counter-pressure is at 80 mm., of mercury. Then the same subject takes a hot bath, and, on coming out of the bath, without dressing, has his pressure taken again. As a result of the bath the amplitude of pulsation was increased enormously, while the blood-pressure was diminished—it was then only 60 mm., of mercury—as was demonstrated by the most favorable degree of counter-pressure. Hence, he would have made a serious error if, on the basis of the enlargement of the pulse, he had inferred an increase of pressure. It is absolutely necessary to determine the most favorable counter-pressure in order to compute the pressure. This is the method with which it is necessary to begin.

But, on the other hand, this method entails great practical difficulties; we observed three: 1., it is slow, it requires a manipulation which lasts at least 1 to 3 minutes; 2., it produces very clear sensations in the fingers of the subject as the pressure is changed, and these sensations might disturb his attention in a manner prejudicial to the experiment; 3., it causes, by the changes of pressure, certain excitations which may produce reflex phenomena, such as vascular constrictions, in the fingers.

The first of these difficulties, the most serious, shows itself when we study the changes of pressure produced by a phenomenon which lasts only a short time; for example, the result of the concentration of the attention, of a mental calculation, or of a deep breath. Suppose we wish to know whether a mental calculation increases the blood-pressure or not, the time needed to take all the pressures from 0 to 120 is at least 1 to 3 minutes. Hence, it is necessary to see that the mental calculation lasts just so long, and that is not always easy. Moreover, the method can only indicate the blood-pressure at the moment when, by trial, we reach the most favorable counter-pressure. We do not know exactly what the pressure at the beginning of the intellectual labor was, nor how it changed during the progress of the experiment. All this shows that this method is insufficient. But such as it is we believe it to be indispensable, 1., to show whether the average pressure has increased or diminished, 2., to indicate in millimeters of mercury the value of the change of pressure.

In Kiesow's article on the effects of psychic excitations, studied by means of Mosso's Sphygmomanometer,¹ the author almost never employed this method; he indicates it only once. He employed it on a Privat-Docent to study the blood-pressure after mental work, and noted a deviation of pressure equal to 8 mm.² But he does not give the tracing, and the experiment indicates only what occurred afterwards, and not what occurred during the mental work.

B. The second method, much the shorter and more convenient, consists in registering the pulsations with a constant pressure of the manometer, and then producing the mental work and other psychic phenomena studied, without changing the pressure of the manometer. Thus by the first method we change the pressure successively, in order to register the maximal amplitude of the pulsation, while by the second we leave the pressure constant, and observe simply the changes of amplitude of the pulsation which the mental operation produces. For example, we begin by registering the pulsation of the fingers under a pressure equal to 50 mm. of mercury; then without touching the piston again, we ask the subject to make a mental calculation and observe whether there are any changes in the pulsation, the pressure remaining always at 50 mm. as before the calculation.

What is the advantage of this method? It will be seen at once: 1. We seize the first modification produced by the mental calculation, and all that occurs at the beginning, the middle, or the end of the process. 2. We do not distract the attention of the subject by changes of pressure in the fingers. 3. We do not produce reflex vaso-motor phenomena by changes of pressure.

But this method cannot inform us whether the pressure has changed or in which direction it has changed. It shows us merely—when it shows us anything at all—that the pulse has changed in amplitude. Now, it was mentioned above that the changes of amplitude in the pulse (the case of Dr. Columbo is an example of this), are not a constant sign of changes of pres

¹ *Arch. ital. de Biologie*, 1895, xxxiii, p. 198.

² *Op. cit.*; p. 207.

sure; hence it may be that when the pulse becomes stronger, the effect is due to a relaxation of the arteries, to a diminution of blood, or in any case to some cause other than an increase of pressure. Hence, before employing the second method we must employ the first, which shows with certainty whether any change of pressure occurs and in which direction it occurs. The first method gives the principle fact and the second the details. The two are complementary.

There remains one very important question: when the second method is employed, what pressure should be chosen as constant? There is a very simple way of determining it, *viz.*, to compare the two tracings of increasing pressures made, the one in the normal state, and the other during the phenomenon which we wish to study, such as mental calculation. The comparison of these two curves allows us to decide for what counter-pressure they differ most. If the maximal difference is, *e. g.*, at the counter-pressure of 120 mm. of mercury, it is that counter-pressure which we must choose for the method of constant pressure, since it is that which is most favorable for the differentiation of the two curves.

We must here make a criticism of Kiesow's work. This distinguished author has not determined the most favorable counter-pressure, and in his experiments with constant counter-pressure, he has always chosen the most favorable counter-pressure, that which gives the maximal amplitude of pulsation. We do not find in his work any justification of his choice. This choice, it must be admitted, is not the most fortunate, as is shown by numerous tracings (1 to 6) inserted in his work. In these tracings it is impossible to see whether the mental calculation has had any influence upon the blood-pressure, since the pulse-amplitude shows no change. It is possible that among the individuals whom he has studied, intellectual labor produced no effect upon the pressure. We cannot tell. But we believe it more probable that the negative result reached by Kiesow is due to the fact that he chooses as the constant pressure the most favorable pressure; for, on the one hand, the maximal difference between the two curves for rest and mental labor does not occur, according to our own experiments, at the most favorable coun-

ter-pressure; and, on the other hand, we observed constantly among the subjects in whom intellectual labor produced an increase of blood-pressure, that this effect is not noticeable when the most favorable pressure is taken as the constant pressure, but is most clearly noticeable when the strongest counter-pressure is used. These considerations lead us to believe that Kiesow has committed a technical error, quite excusable indeed in an author who is not entirely familiar with the graphic method.

II.

Our experiments were made upon three subjects, but principally upon a young student of psychology, 23 years old, and in good health. We will speak simply of the experiments made upon him. Last year we made on him numerous experiments on the effect of mental labor on arterial circulation, and hence he is able to observe himself and concentrate his attention. His pulse, compared with the others whom we examined, is weak, but his pulsation is well formed; when he makes a mental effort there occurs in him almost always a fine and quite radical vascular constriction, with a diminution in the size of the pulse-curve; then the vascular constriction disappears and the tracing returns to its normal level; this return to the normal may take place before the mental effort of calculation is completely finished, particularly if the subject has been given to solve a very complicated problem which requires much time. There is, besides, during the calculation an acceleration of the heart and of respiration. These experiments do not show whether intellectual work increases the blood-pressure. The vascular constriction of the capillaries which we observed tends to relieve the pressure, as does also the acceleration of the heart, but since we do not know what the work of the heart, what its force was, during this time, we cannot say certainly that the pressure has increased.

The Sphygmomanometer relieved all doubt. At the beginning, we employed the first method of experimentation, which consisted in taking the pulse under increasing pressure from 0 to 140 mm. of mercury; this test is made at first while the subject is in a state of rest without excitement or preoccupation

of any sort; then the same experiment is made while the subject is absorbed in a difficult mental calculation; in this way two curves are obtained for comparison; the difference of the two curves can be attributed to the intellectual labor unless some chance circumstance, as an emotion, a shiver, etc., prevents the two experiences from being comparable. This double test was made on our subject at five different times so that we obtained ten curves which may be compared in pairs.¹ The change of pressure from 0 to 140 mm. was made each time very slowly by hand in an almost insensible manner, lasting almost always the same length of time (two minutes and a half); the experimenter regulated the speed of his movement by means of a seconds watch. The pulsations of the mercury column were not written by means of a float on a revolving cylinder (Mosso's method), but were transmitted by means of air pressure to a Marey tambour; a very small escape² attached to the transmission tube prevented the pen from becoming displaced through the influence of the sinking back of the column or mercury (produced by the piston), so that the pen traced at approximately the same level the pulsations of the column of mercury, although the latter was at different heights, varying from 0 to 60 mm. We found this arrangement much more advantageous than that of Mosso, which gives the tracings in steps; by means of ours, one may perceive more easily the gradual changes of amplitude.

The mental calculation was to last for about two or three minutes: we did not give the subject a single calculation, because it would have to be very complex and very difficult to last so long, and the subject, who had no special talent for this sort of exercise, would have become confused and lost the figures; and in the end, we should not have been able to get the strong and concentrated attention which we wished to study. It seemed better to give to the subject a series of easy multiplications; as soon as he had finished one, he gave the answer and was immediately given a second, then a third. In general, during the two or three minutes that the experiment lasted, the subject

¹ A much larger number of experiments have been made since these lines were written.

² On the regulation of graphic tracings by means of an escape, see the *Année Psychologique*, II., p. 776, 1896.

made three multiplications, each of two figures into two figures. As he was very much interested in the experiments, he always made a vigorous effort, as is shown by the correctness of the answers given. He closed his eyes, knit his brows, and leaned his head a little forward.

We measured exactly the amplitude of the pulse for the ten experiments: they were made at the same hour and under strictly comparable conditions.¹

We subjoin the results in Table I.

TABLE I.—AMPLITUDE OF PULSE UNDER DIFFERENT PRESSURES DURING A STATE OF REST AND DURING INTELLECTUAL WORK.

Pressure.	Without Intellectual Work.					With Intellectual Work.					Average Without.	Average With.
	1	2	3	4	5	1	2	3	4	5		
20			0	0.5	1			0.5	0.5	0	0.5	0.5
30			0	1	1.5			0.5	0.5	0	1	0.5
40	1	0.5	0	2	2	0.5	1	1	1	0.5	1	1
50	1	0.5	0.5	2.5	3	0.5	1	1	1.5	0.5	1	1
60	1	1.5	1.5	4	5	1	1.5	1.2	1.5	1	1.5	1.2
70	2	2	2	3	4	1.5	2	3	3	1.5	2	2
80	2.5	2	5	5	5	2	3.5	4	4	2	5	3.5
90	2	2	3.5	4	4	2	1.5	3.5	3	2	3.5	2
100	1	1.3	2	2	0.5	2	0	4	3	2	1.3	2
110	0.5	1	0.5	1.5	0.5	1	0	2	1.5	1.5	0.5	1.5
120	0		0	0.5	0	0.5	0.5	1	0	0.5	0	0.5

Explanation of Table I. The five first vertical columns on the left indicate the amplitude of the pulse during five experiments in the normal state; the amplitude is measured in mm; we begin with an amplitude of 20 mm. of mercury, then 30, then 40, and so on up to 120. The five following vertical columns show the amplitude of the pulse during a series of mental calculations at all pressures from 20 to 120 mm. of mercury. Finally, the two last columns show the average² of the results

¹The pressure varies with the hours of the day. We always made alternately the experiments of rest and intellectual work, in order to keep them in comparable conditions.

²We give, not the arithmetical mean, but the *median value*, as indicated by Scripture (*PSYCHOLOGICAL REVIEW*, II., 1895, p. 376, and *Année Psychologique*, I, 1894).

obtained from the experiments in the normal state and during intellectual work respectively.

Let us look for a moment at the left side of the Table (I), containing the results of individual experiments. We are struck with the irregularity of certain series of figures. The amplitude of the pulse does not increase regularly up to the most favorable counter-pressure and then regularly decrease. In experiments 4 and 5 in the normal state, *e. g.*, the figures make several quite unexpected jumps. This is due to the fact that the subject is not an automaton; he has had during the experiment vascular constrictions and changes of blood-pressure which have changed the trend of the curve. Similarly in experiment 2, during intellectual labor the subject has had suddenly near the end of the curve at a counter-pressure of 100, a reflex vascular constriction caused by a rather strong emotion (he noticed that he had given a wrong answer to the problem set). These experiments show that we must multiply the experiments and only preserve the average results, in order to eliminate the sources of error.

Let us now compare the averages from the last two columns of Table I. The differences are evident. First, the curve of pressures of the state of rest has a greater amplitude than that of intellectual work; the maximum amplitude of pulsations in the first curve is 5 mm., that of the second is only 3.5 mm. There has evidently occurred in our subject during all the mental calculation, a diminution of the pulse which results from a vascular constriction that is more or less marked. The position of the maximum point in the two curves is about the same, at 80 mm. of mercury, and, if this fact alone were taken into consideration, we might conclude that the blood-pressure was not modified; but we must hasten to add that when the pressure was increased beyond 80 mm. the two curves acted very differently. The pressure curve for the normal state decreased rapidly; at 100, it fell to 1.3 mm., and at 110 it was practically suppressed; on the other hand, the pressure curve for mental work withstands the stronger pressures more vigorously, notably pressures from 100 to 120. In short, here are two cases which coincide in their maximum, but which differ very much in their resistance to strong pressure. We think that this resistance must be taken

into consideration; when one pulse withstands a counter-pressure of 120 and still records, while another pulsation of the same amplitude is checked by this counter-pressure, we must recognize that the former pulsation corresponds to a stronger arterial tension. We are thus led to set aside Mosso's criterion in this particular case, and to accept that of Marey.

The tracings which we have obtained, together with our numerical results show clearly the essential difference between the two pressure-curves.

In determining the complexity of this phenomenon, be it understood, it would be difficult to take account of it with a clinical sphygmometer of Bloch's type, a method which consists in obstructing the radial artery until the finger inserted between the syhygmometer and the radial no longer perceives the beatings of the latter. The experimenter would have to be very skillful in taking exact account of the constriction produced by intellectual work, which decreases the amplitude of the pulsation, and to perceive that in spite of this diminution, which ought to give to the exploring finger a new sensation, the pulsation has greater resistance.¹

III.

From what precedes, we conclude that, with our subject, a pressure of from 100 to 120 completely suppresses the pulsation of a state of repose as also that of a state of intellectual labor. This observation will help to guide us in the second series of researches, where we will employ a constant pressure; it is clear that to make apparent the difference between the circulation in a state of intellectual labor and that of rest, it is this counter-pressure of from 100 to 120 which must be chosen.

In order to remove all doubts we have made a counter-test in the following manner: Seven times in succession our subject made a mental calculation having his fingers subjected to a constant pressure, and each time the pressure chosen was different; the results also were very different. With a constant pressure of 40 mm., there was no modification produced by the

¹Féré, who made some use of the sphygmometer, noticed that when the artery contracted, *e. g.*, under the influence of cold, the apparatus gave only erroneous indications (*Pathologie des émotions*, p. 14., note).

fact of mental work; with a pressure of 60, the same negative result; with a pressure of 70 mm. there was a slight augmentation of pulsation; at 80 mm., there was again a slight augmentation, not measurable, but visible to the eye; at 100 to 120 the augmentation is very clear; it varies from simple to double; at 140, all pulsation was suppressed. This shows us very well that the constant pressure chosen ought to lie between 100 and 120. These experiments are a confirmation of those made before: they show anew that if we choose the most favorable counter-pressure, we may obtain results which are as completely negative as those of Kiesow are.

Now let us take a counter-pressure of 110. We first register the pulse with this pressure for about a half minute, then tell the subject to commence a mental calculation. While he is absorbed in his work we watch the pressure with care; for when we give a heavy pressure with the Sphygmomanometer, it tends to diminish; and in order to keep it equal, it is necessary to give a stroke with the piston from time to time. This slight correction ought to be made very gently, so that the subject does not experience any new sensation in his fingers, and does not perceive anything. Excepting this slight cause of error, for which we ought to watch, this method of experiment is much easier than the preceding; one does not disturb the person in the experiment, and, moreover, the change of pressure in the hand is seen in the tracing as soon as it is produced. The experiment carries with it a kind of sensible evidence; as soon as the mental calculation begins, there is an increase in the pulsation.

This experiment was made 11 times upon our subject, always with analogous results. We gave him multiplications of two figures by two figures, and immediately he began the operation in his head. The first three or four pulsations which register themselves are usually of the same character as the preceding, sometimes they are slightly shortened, an effect which is probably due to the vascular constriction which is habitual with this subject at the beginning of intellectual work. Then the pulsation increases, it doubles in size, or becomes twice and often three times as great. This increase in ampli-

tude maintains itself, in general without increase or diminution, and with great regularity during the whole of the mental calculation; when the subject has found the solution and has given it, there is no sudden diminution of pulsation; it may retain its amplitude without change for 15 seconds, sometimes even longer; then the pulsation begins to diminish very gradually; finally it recovers the same amplitude that it had at the beginning of the mental calculation. This return to the original condition is a very significant fact for us, since it shows that the change in amplitude of the pulsation is not due to the apparatus, but to the physiological condition of the subject.¹

A. BINET.

¹This is part of a more general study which I am making on the physiological expression of thought, including also my experiments with M. Courtier on capillary circulation and respiration. Some of the results will appear in April, 1897, in the third *Année Psychologique*.

DISCUSSION AND REPORTS.

LANGUAGE AND IMAGE.

With reference to the understanding general concrete terms, like telephone, tree, things, etc., it would be a very natural supposition that it would only be done by imaging. Indeed, what else is or can be the knowing the meaning of words for sensible objects other than a connecting word with image? and the better the word is understood would there not be the more definite imaging? The natural supposition is that the whole function of a vocal sign of a sensible is to call up that sensible as individual or group to the mind, *i. e.*, the production of an image, and that if a word signifies a thing or things it can only do this through image.

However this very natural idea that the understanding of a sense word must be through sense, seems hardly borne out in practice. Did the reader who understood the first sentence of this paper have any imaging when he came to the words 'telephone, tree, thing?' And does he not understand this last without image? A little reflection assures you of no trace of image, and yet you read the sentence with perfect understanding. By far the great majority of readers at least will find this the result of their introspection. And further it will be said that the better the word is understood the less imaging is required till at length there seems to be none at all. The man who is perfectly familiar with the telephone, both the word and thing, does not need to image as does the farmer's boy on hearing the word. And this appears to be true of both common and proper names. Most men on hearing the very familiar name of a near relative as in casually saying 'my wife,' 'my son,' form no image, but on hearing name of some one whom they have seen but once or twice the understanding of the word implies very definite image. Thus if some one asks, 'is your wife here?' and another, 'is Mr. Penn here?' (an acquaintance seen but once) you answer understandingly at once in the first instance without definitely imaging, but not so in the later case. Hence we have the paradox that apprehension of sense meanings is most perfect when senseless.

The interesting problem of how we understand the meaning of ob-

ject names without conscious object-reference is certainly not solved by Mr. Stout's remark that 'imageless apprehension' merely is 'the power to distinguish the apprehended object from other objects,' though 'the constituent parts of the object cease to be discernible.' (Analytic Psychology I., p. 84.) But this surely is not real imageless apprehension, but only the last stage in the imaging apprehension, that is, when we need but one distinctive mark in referring the word to the thing. Here the image is reduced to a single element, but is as real an image as ever. But it is obvious to an ordinary reflection that in common conversation and reading we are continually understanding words, and yet not having, so far as we can see, even a trace of image of mere distinctive mark or generic aspect.

But I think the best clue to the mental process in understanding sense words without apparent imaging is given by a study of cognitive process as a whole, by comparing the understanding meaning of a word with the understanding meaning of a thing. All knowledge of objects is interpretation, is a getting at meaning, and all objects as known are thus signs, and in a large sense constitute a language. In knowing what a telegraph instrument is when I see it I am aware of its significance, as contrasted with a Hottentot, to whom the object would have no more meaning than the English words designating it. And the interpreting becomes easier with successive experiences of the same things until they are at length known without any interpretation being consciously applied. Every time I see a chair I know it to be such, though I do not consciously image it in its use as a chair. I have learned to know it so easily and quickly that the knowing act becomes unself-conscious and so unrememberable, but continues as real act, for it is evident that in the most casual notice of a chair one really appreciates it for what it is. When you go into a drawing room you will both know the chairs and sit down in them quite mechanically, though if you see some strange piece of furniture, which you have to identify as chair, then there is self-conscious identifying interpreting process which is rememberable knowing.

Now the knowing meaning of a word and thing are quite parallel activities; indeed, word is really kind of thing. If a very common thing like chair becomes so well known as to be subconsciously known, we might expect the same of its name. And in fact, let the reader reflect on his understanding of the previous sentences where this word chair occurred, and he will hardly find in any case that he has been conscious of any imaging process and yet he is sure he understood the meaning. The name chair has, through practice, be-

come so familiar and the process has been made so quick, easy, and abbreviated as not to be in the least intruded on self-consciousness. In general we suspect that the objects which are subconsciously known will have names subconsciously known, that the imaging interpretation is in both cases carried on, though not rememberable. The law of habit, that we get to doing acts so well we are not aware of acting, applies in both the knowing things and names; and the general results of common knowing seems to confirm this. This habit theory is plainly quite different from Hobbes' theory of habit and understanding.

Of course, in the nature of the case, we cannot get direct proof of unrememberable mental process. Yet an illustration of the effect of habit upon knowing is seen in the perception of distance, which we have learned so well in early life by a real judging, which later becomes unself-conscious process; and hence the ordinary theory of space perception is intuitional. Again, another illustration is in reading as a very rapid subconscious spelling, and in thinking as unself-conscious mental pronouncing of words. The poor reader consciously spells, the better reader takes in the spelling of the word and cognizes it at a glance, and is unconscious of any process, and the very best reader grasps by sentences and is unconscious of either letters or words as such. Similarly the poor thinker talks aloud or moves his lips, the practiced thinker only mentally pronounces, and the best thinker is unconscious of using any words. Yet it is undoubted that thought, which has once thoroughly learned words as its instrument, never after becomes really wordless. Thought by means of words cannot get beyond words.

A more direct evidence of subconscious process than these analogies is this: that we sometimes come upon a very common word whose meaning we have to definitely search for, and we rummage our collection of images till we find the right one, and the old faculty is reëstablished. By a constant imaging of thing for word and word for thing, a perfect coördination is formed which is carried on in an under-consciousness—that is a consciousness of which we are not and do not need to be conscious. Again, an evidence of subconscious imaging in understanding meaning is this: that when we have read understandingly several sentences of general sense terms without being conscious of imaging, but are then asked to state the substance in our own words, we immediately begin to marshal the images which we did not directly connect with the words, but which we yet seemed in some way to have had.

Practice, of course, tends to abbreviate the image, and we become satisfied to only partially realize meaning with a dim sense of an indefinite realizability, which for practical purposes we do not need to carry out. There is generally felt to be a great potentiality in the word which we do not stop to measure. When we are told that it is a thousand miles from New York to Chicago, we may understand well enough with a *minimum* of realization, but which we know at the time can be indefinitely increased. A word is a machine which easily moves an unimagined, yet imaginable mass of images and ideas, and it is the very office of words to give us the practical manipulation of these masses without recourse to any but the most general imaging reference, and that often subconscious, yet with a constant sense of unrealized but realizable potentiality.

In helping us to realize the hidden image force, our main reliance is in poetry, which may be defined as the art of using words in such a way as to awaken image to full life. Poetry partly accomplishes this by a special vocabulary of its own, and partly through the dextrous throwing of the word into a new and more striking position, as by inversion, metre or rhyme, or mentally by a trope, and so leading the mind to image.

Poetry revivifies language by bringing out the latent image or inserting a new one. Take Tennyson's expression, 'The rainy Hyades vex the dim sea.' Here each word in itself may evoke no image, but the combination and the figurative use call up a most vivid image, and that vast reach of the imaginable wherein poetry chiefly lies. Even a bare connective, as 'and,' has its latent image evolved when used emphatically, as in the line:

'With rocks, and stones, and trees,'

where a dim visual 'more' is evoked. If the poet uses only the common words of prosaic life, as did Wordsworth, he must have great skill to attain the imaginative effect, and the more common the word the harder it is to give it sensuous force.

We have thus far remarked only upon the understanding of the meaning of the concrete terms, more or less general, but language is plainly more than notation of sensibles. Some words, like 'signify,' 'idea,' for instance, are purely intellectual words, and any infusion of image but distorts the meaning. And many words also relate rather to pain, pleasure and emotion than to the sensible in any form, and must thus be understood. In fact, the first expressive vocal utterance was doubtless a cry of pain, animals otherwise dumb giving in great pain a squeak, which of itself conveys no sensuous image. Primi-

tive language is not a name, but a cry. We know immediately what is meant upon hearing a cry, because it awakens in us some of the painfulness for which it stands. One to whom pain was wholly unknown would be as unable to interpret a pain note as a blind man to interpret the words red, white and blue. It is very probable also that what we take to be vocal signals indicating food are, with the very lowest animals, at least, only signs of eating pleasure, and so do not convey real image of object as food. The eating act, as is evident from observation of very young infants, is carried on at first with little or no consciousness of what is eaten. It might even be maintained that animal language is never properly denominative, and only with the higher species does it become even indirectly denominative. Mere emotion words, like joy, sorrow, hate, etc., are also plainly understood without sense image. Nothing is really imagined, no real objective reference is really made, but the words in their isolation are understood wholly by subjective realization. In some very slight measure the understanding of all pain words gives pain and all pleasure gives pleasure, that is, in revival, just as the knowing the meaning of sense words implies revived sense—that is, image. In understanding meaning of the words joy, sorrow, you experience at least a faint joy and sorrow.

We conclude then that language, as an indicator, can only indicate by suggesting to our consciousness what is indicated, as object, thought or feeling, even in most summary and unself-conscious form to which it is brought by practice, and from which it may be rescued by poetic art to its primitive vividness.

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UPRIGHT VISION.

Dr. Stratton's paper in the last number of the *PSYCHOLOGICAL REVIEW* on 'Vision without Inversion of the Retinal Image' calls for some criticism. The first sentence in it shows that he means to discuss the problem of upright vision, and in the same paragraph he announces his purpose to examine the projection and ocular movement theories of this phenomenon. But the course of his experiments and arguments shows, not only that he had no right to talk about upright *vision* in this connection, but that he has not even understood his own problem. Nor does he in reality discuss the two theories mentioned, much less throw any light upon them.

His phrase 'vision without inversion of the retinal image,' espec-

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ially when taken with the first sentence, where 'upright vision' is mentioned as the problem, is the main source or evidence of his confusion, as it shows with the later experiments that he has not distinguished between visual perception and tactual or motor adjustment to visual perception. The problem in upright *vision* is to determine how we see objects in a certain way when the image on the retina is inverted. This problem remains the same throughout all of Dr. Stratton's experiments. This is noticeable in the very statements he makes about the effect of the glasses upon what is seen. He concedes the inverse relation between image and apparent object after the glasses are put on, but does not see that this is only a reproduction of the normal relation, and that in so far as *vision* is concerned there is nothing anomalous in the effect. But instead of discussing this question, or getting true upright vision after inverting the normal image, he goes on to study the influence of experience upon motor (not ocular) adjustment to these new conditions and the influence of memory images upon our notion of uprightness. Very well. But this is not a problem in upright *vision* at all. It is merely a problem as to the influence of memory and association upon muscular adjustments, and these *not* ocular movements at that. Dr. Stratton has not analyzed his problem in the least, and his whole discussion will only lead the unwary to think that he has shown the effect of experience, muscular experience and adjustment upon the *ocular* perception of uprightness, when the very fact that he concedes the inversion of the apparent object under the glasses proves that the visual process is as before and is not affected by the foreign factors of either memory or touch. His attempt, whether implicitly or explicitly avowed, is to show that inversion of the retinal image is not necessary to upright vision, but all that he actually shows is that this inversion is not necessary to correct tactual and muscular adjustment and the formation of new judgments for motor movements. That is to say he only shows that we have to invert the memory images at first by an effort of will in order to secure correct movements, and then experience establishes a line of spontaneous connections as prompt as the old ones. But this is no more the problem of upright *vision* than it is one of upright audition. Hence to talk about the projection and ocular movement theories in this connection is simple nonsense, if I may be allowed to use such severe language, and only betrays a misconception and misrepresentation of the problem.

When Dr. Stratton talks about 'vision without inversion of retinal images,' and then discusses motor and tactual adjustments, he ought

to have seen either that he was not talking about vision at all, or that the phrase 'without inversion of retinal images' was an equivocal one and leads to complete confusion. It might mean 'without inversion' in relation to the *real* object, which after all is not seen at all, or 'without inversion' in relation to the *apparent* object, which is described by Dr. Stratton himself as representing in fact the inverse relation of normal vision. There is no anomaly in the latter case, which only shows that he does not mean to assert the absence of inversion between retinal images and the apparent objects under the conditions described. Here the visual problem is not altered, but taking the phrase in the first sense, 'without inversion' in relation to the real object (which is not *seen*), the problem is tactual and motor. If at any time he could show an instance of a symmetrical and not an inverse relation between retinal images and real or apparent objects, he might reasonably enough imply or assert that upright vision with inverted images is not an organic but an empirical process. Until he does this, such experiments as he describes in his paper are irrelevant to the problem.

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PSYCHOLOGICAL LITERATURE.

History of Philosophy. ALFRED WEBER, Professor in the University of Strasburg. Authorized translation by FRANK THILLY, from the fifth French Edition. New York, Scribner's, 1896. Pp. xi + 640. \$2.50.

One need not hesitate to pronounce this the best history of philosophy for use as a text-book, and for the purposes of the general reader. In translating it, Professor Thilly has rendered a not less valuable service than in his translation of Paulsen's *Introduction to Philosophy*.

Professor Weber has an admirable faculty of exposition. He knows how to select out of a mass of details the points most suitable to his purpose, and to present them in a lucid, graphic, and interesting way. His work is less suggestive and original than that of Windelband, but has the advantage over it of a much more simple and natural method. He is less detailed in his discussion of systems than Falckenberg, but the latter treats only the modern period, while the entire development of philosophy, from the beginning of Greek speculation to the present time, is brought within the six hundred pages of this volume. The sense of proportion is, in the main, good, though Greek philosophy receives less than its due share of attention, being allotted scarcely more than one-quarter of the space. Too much can hardly be said in commendation of the literary skill which handles the vast body of materials, with which a general history of philosophy has to deal, in such a way that one without previous knowledge of the subject need have no difficulty in following the narrative with pleasure.

It may be questioned whether the two-fold division, 'the Age of Metaphysics' and 'the Age of Criticism,' under which both ancient and modern philosophy are treated, is best adapted to exhibit the relations of the history. The movement of thought may naturally be conceived as three-fold: first a period of construction or interpretation, then a reaction of sceptical reflection, and then a restatement, in more systematic form and from a more comprehensive point of view—this restatement becoming, in turn, the subject of critical analysis, followed by fresh attempts at construction. Windelband recognizes these stadia in his distribution of the philosophy of Greece proper

into the 'Cosmological,' 'Anthropological' and 'Systematic' periods, and in his treatment of the post-Aristotelian, or 'Hellenistic-Roman,' period as one of revolt or decline, and also—largely because of the religious elements in Stoicism and Neo-Platonism—one of positive constructive activity. This is a more discriminating and helpful outlining of the subject than one which includes, in a summary and heterogeneous way, the whole body of thought, from Protagoras to Proclus, under a single rubric. The parallelism between ancient and modern thought is brought out very clearly when we consider each as following this law of development, but the affinities and analogies between the two are obscured when after Descartes, Spinoza, and Leibnitz, regarded as constituting the 'Age of Metaphysics,' we find all other philosophers, from Locke to Spencer, enumerated under the 'Age of Criticism.'

It is hardly just to complain of omissions rendered necessary by the plan of the work. It is, however, of interest to observe that some aspects of the subject are treated more fully than others. The relation between philosophy and science is accorded a good deal of prominence, the speculative bearings of the theory of evolution, in particular, forming the subject of one of the most interesting sections of the book. On the other hand, the relation between philosophy and theology is passed over lightly. Little or no mention is made of the English Deism, the German Illumination, Hume's *Natural History of Religion* and *Dialogues*, the Kantian theory of religion. The effects within the domain of dogmatic and critical theology of Hegel's philosophy of religion are very imperfectly indicated. The highly important philosophic theologian, Schleiermacher, is disposed of in a single sentence. A noticeable omission is that of the series of English writers upon ethics following Hobbes. Political philosophy receives little attention; in speaking of Hobbes, Spinoza, and Locke, their ideas in regard to the origin and nature of the state are not noted; Grotius is not mentioned, or Montesquieu, or Rousseau. The sketch of contemporary philosophy dismisses so influential a name as that of Lotze with a bare mention. These *lacunæ*, however, and others that might be enumerated, are not properly occasion of complaint. It should be said that the footnotes make up, to a considerable extent, for the omissions of the text, since they furnish, in the case of all important authors, sufficiently full bibliographical references. The translator has added much to the bibliography and has contributed an index.

A feature of this work which renders it particularly suitable as an introduction to the subject is the appreciative and positive temper in

which it is written. A historian of philosophy who conceives it to be his function to convict every great thinker of as many errors and inconsistencies as possible is as gravely at fault as the literary critic who occupies himself only with the faults of his author. The discovery of truth and beauty to those who are liable to overlook them is a far more valuable service than the mere exposure of their opposites. Every one who has observed the painful sense of disappointment which is so often the first result of a study of the great speculative systems will appreciate the wisdom of such a treatment of the subject as is indicated in remarks like this: "To the argument (page 594) drawn from the perpetual disagreement of philosophers, we answer that the historian of metaphysics is most impressed with the open or tacit agreement existing between the rival movements and schools. We have discovered such agreement between Plato and Democritus, Descartes and Bacon, Leibnitz and Schopenhauer, Herbart and Hegel." Instead of emphasizing weaknesses, inconsistencies, and disagreements, Professor Weber seeks to exhibit the points of contact between different systems, the elements of truth held by them in common. This is the most effectual way of securing the inexperienced student against the bewilderment and sense of futility which are so apt to overcome him. It is not a defect, but an important merit, of a work which has in view the needs of novices, that it should announce a definite philosophic doctrine. The reader who has patiently followed the long way which European philosophy has traversed may well be reminded at the end that his journey has not been purposeless. A constructive chapter, like that which concludes this volume, is valuable and wholesome, irrespective of the intrinsic merit of its reasonings, as an example of the spirit in which one should study philosophy, and of the fruits one may hope to gather from it.

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The Power of Thought: What It Is and What It Does. By JOHN DOUGLASS STERRETT. With an introduction by J. MARK BALDWIN. New York, Scribner's, 1896. Pp. xiv + 320.

Introductions by better known men for books written by those who are less known are not *à priori* commendable. As a general rule books had better speak for themselves. That there are justifiable exceptions, however, Baldwin's judgment shows in the present case. For here is a book admirable in many respects, but with a title calculated to make it *caviare* to the very people who alone might be expected to

read it. Those whom at first sight the title would 'catch' are again the very ones who would never have the mental patience and courage to read the book through. But if neither the public nor the specially interested read it, it 'falls between two stools.' The association of Baldwin's name will draw the attention of the latter class.

The book is, as I have said, in many respects admirable; it is thoughtful, well-informed and independent in the true sense. There is, too, for the most part, a certain naïve charm about the author's style and his way of putting things. But in spite of this, there is much that is uncouth and barbarous in expression scattered through these three hundred pages—much that is more forbidding than the technicalities of the professional psychologist. After all, when one is writing on science it is hard to avoid technical terms. Those who will read Sterrett's book would read it with more satisfaction if there were a more precise and exacting terminology. The last issue of this REVIEW puts the pertinent question: "If one is to traverse a desert, why not ride a camel?" This, to the reviewer's mind, is a fault of the book, causing the author, despite what he says in his preface, to be often diffuse and at times tiresome. Moreover, why wrest from technical terms their customary meaning? Why, to take one instance, keep on talking about physical sensations? "A sensation is but a physical impression in the sensorium, not an idea, not knowledge, not consciousness" (p. 37). For this meaning see also pp. 21, 23, 26, 28, 29, 48, 50, 51, 52, 57, 64, 80, 82, etc. Is it not surprising, then, in the face of all these references, to find the following language on p. 67: "Here, then, is something that touches its soul with a sensation, or a feeling." Are sensation and feeling, then, in any sense equivalents? If they are we are compelled, in trying to harmonize pp. 37 and 67, to consider feeling, too, as physical and extra-conscious! But perhaps the author will say that the context shows that sensation and feeling are not intended as equivalents. Why, then, did he not write *and* instead of *or*? We read again, on p. 191: "And I affirm that the tone and stress of sensation is as much set up genetically by mind as by the exterior potency." Is this not implying that sensation is a conscious as well as a physical perturbation? Can one read such passages without a sense of dumb dismay at this playing fast and loose with terms which ought to have something like a definite meaning?

Notwithstanding, many such defects in execution, Sterrett's general purpose is praiseworthy. His main thesis that mind or thought is power (his popular way of expressing the psychologist's 'mental dynamogenesis'), is worked out from the beginning to the end of the

book, with a concentrated and yet comprehensive purpose which ought to prove suggestive to the specialists themselves. This note is sounded in the very first sentence of the first chapter. "To be a free agent man must have the ability to achieve his freedom." And the underlying aim of all the chapters seems to be to prove that he has this ability. No stone is left unturned in the attempted proof of this. It is made plain to the author's mind by a true reading of the conscious life of our sub-human brothers; it is worked out again in an ingenious bit of genetic psychology, where the beginning and organic development of the child's powers are the object of analysis and interpretation; and it finds its final corroboration in the normal life of the mature man.

The most impressive parts of the book to those who are more philosophically than psychologically inclined will be the chapters in which the analysis brings us to close quarters with 'free will.' However, familiar the reader may be with the ins and outs of this question, he will here find much that is instructive. The more fundamental issues are dissected out of the body of the question and stated with a clearness hard to find rivaled. See, for example, Part I., Chap. IV: Mind and Brain, and Part IV., Chap. XIX. and XX. In Chap. IV. are to be found some of what Baldwin calls in his introduction, 'points of view * * * * of the latest scientific investigators.' The reading of it calls to mind at once the names of Romanes, James, S. Hodgson and Baldwin. (See Baldwin on 'Consciousness and Evolution' in the May, '95, number of this REVIEW.) What the author says about motives is also worthy of special mention—his interpretation reminding one strongly of passages in Green, James and Baldwin. The conflict of motives is not a conflict between separate ideas, each with a distinct activity of its own, and exploding its own gun to compel submission from the others. Such a conception is as imaginary as that 'chimæra bombinans in vacuo'—the freedom of indifference. A motive has no independent existence and means nothing if it is not 'a name for a partial expression of the nature of the agent.' Very similar, too, to Baldwin's and Hodgson's is his description of the process through which an end passes into volition.

But in spite of all this, which one may cordially applaud, there are occasional lapses from philosophic grace. For example, one fails to feel the force of his method of appeal to 'facts at first hand' (pp. 258 and 260). The writing here is below the author's standard; he seems to be regarding facts as if they were stones to be picked up by the mere reaching out of a hand. What is a fact? Why did Sir

John Herschel say that there were more false facts than false theories in the world? Would not a little idealistic analysis do this kind of realistic appeal a great deal of good! Besides, doesn't the author know the literature on the distinction between 'the sense of freedom' and 'the fact of freedom?' Has he never read Martineau, Hodgson, Chalmers and Mill! Or, does he ignore them? Outside of this and a few other lapses, these chapters on freedom are suggestive and convincing.

The book as a whole teaches well the lesson "that we know it" (the world) "not with, as it were, a quasi-detachable intellect only, but with our whole living energy; that we know in so far as we act, nay, that ultimately, only as we will, as we put forth activity, as we act, can we claim fully to be." (Introduction to Croom Robertson's *General Philosophy*.)

ROGER BRUCE JOHNSON.

MIAMI UNIVERSITY.

The School of Plato. F. W. BUSSELL. New York, Macmillan & Co. 1896.

The author of this book shows unusual insight into the spiritual forces that were at work in that movement of Greek speculative thought, in which Platonism stands central. His aim is to interpret the movement from a special point of view. The title of the book is justified by the discussion as a whole. The author fears in his preface that he may be charged with superficiality and tiresome iteration. He is not open to either charge, though, as to the first, a few more details in places would have been acceptable. He finds his starting point and criterion of interpretation in a study of the philosophical and religious movements of the Roman imperial period, which had their rise in the mingling together of the elements of Greek, Jewish and Oriental culture. These movements profoundly influenced and were in a sense absorbed by Christianity, with which they came in contact. In the union of intellectual and mystical elements, in the current of speculation, and in the aim of the period which was a search for the blessed life, the author sees the true method and end of all philosophy.

The central motive of philosophy, according to Dr. Bussell, is individualism; the self-assertion of the free spirit of man against the universal, whether in the form of nature or society. With this idea in mind the author follows the stream of Greek speculation from its source. His thesis is the rebellion of the individual; his uprising against an environment that seeks to crush him, or, at least, to rob him of his freedom and make him a slave of the universal. The

beginning of speculation is the awakening of the individual, and the stages of the Greek movement mark the individual spirit's struggle against nature and society, and, in the later theosophic stages, against an absolute which threatened to swallow up the individual life. According to the author, the supreme end of philosophy is practical, the establishment of a *modus vivendi* by which happiness may be secured. The theoretical and logical are subordinate therefore to the practical and moral.

The author finds the highest organ of philosophy in Platonism, in which the process of knowing is supplemented by mystical intuition. The processes of knowledge are inadequate, and here the author is somewhat agnostic; knowing breaks down or falls at the threshold, and recourse must be had to feeling in order to attain the highest truth. It is through this union of thought and feeling that Platonism obtains its grasp on the truths of religion and the immortality of the soul. The author shows, and this is the most interesting feature of the book, how in the later period the final solution of the problem of philosophy in the ancient world was reached by the reassertion in Neo-Platonism of this Platonic synthesis of thought and mystical intuition.

Throughout the theosophic period, and, in truth, since Aristotle, the pendulum of speculation had swung between the extremes of an immanent naturalism which merged God in the course of the world and a transcendent absolutism which removed him beyond the pale of conceivability. Neo-Platonism finds its solution of this problem, and the final one of ancient thought, in the doctrine of emanation which saves the divine transcendence and yet brings God into relation with man and the world, through a chain of mediating beings.

Dr. Bussell characterizes the doctrine of emanation as an honest attempt to overpower the dualism in which stoic cosmogony had ended, but regards the solution as unsatisfactory. What is needed is not so much a theoretic solution of our difficulties as "a divine voice of consolation; something to assure the soul of man of its *intrinsic* worth, of its value in its Maker's eyes * * * something to show us that the practical life and the government of material things is, after all, the highest duty and happiness for us; * * * and above all that God is no palace-secluded sovereign, but a general who fights with us and for us; and this semi-dualistic conception of the efforts, the *painful efforts* of Deity, paradoxical though it may be called, is nevertheless," the author concludes, "a certainty of experience, the supreme consolation and encouragement of the highest thinkers and most devoted believers. And in this lies the significance of the Christian religion."

The author may be characterized as a profoundly Christian thinker who has read and been influenced by Schopenhauer and who is, therefore, somewhat pessimistic, somewhat agnostic and somewhat out of sympathy with modern democratic movements and with what he calls the collectivism of the century. It might be questioned whether the motive of philosophy is not social as well as individual and whether the despair of knowledge need be so profound as it is in the mind of this author; but at all events the theme is treated with insight, the style is stimulating, and the handling of the materials is masterly.

A. T. ORMOND.

PRINCETON UNIVERSITY.

The Development of the Doctrine of Personality in Modern Philosophy. Part I. In. Diss. Strassburg. WM. H. WALKER. Ann Arbor, Mich., The Inland Press. Pp. 80.

This is a careful and creditable study of the subject. The author holds that "the history of the development of the doctrine of personality in modern philosophy is that of a gradual and necessary return to the consideration of the nature and worth of personality from the opposite pole of philosophical thought." The first chapter shows how philosophy "at the Renaissance turned from the inner world to the outer," from the world of scholastic abstractions to the world of reality. Bacon and Bruno "agree that nature is the immediate end of study." Yet indirectly the thinkers of the Renaissance raised the question of personality. "If truth is to be sought for its uses, material or religious, then to whom is it useful, and by virtue of what nature can truth be of use to him?" The second chapter traces the emergence of the problem in Hobbes, Descartes, and the Cartesian school. Political disturbances precipitated the question of "the ultimate unit of society, the legal person." While Hobbes's mechanical philosophy logically implied the denial of personality, his political philosophy found it indispensable. "He not only introduces the concept into modern philosophy, but he also gives it its characteristic modern note. Man is a person because he is accountable for his actions." The Cartesian treatment of the problem is exclusively intellectual; yet, failing to recognize the centrality of personality for knowledge, the Cartesians lose its distinctive character and reduce the subject to the level of the object. The rationalistic dissolution of personality in Spinozism is next indicated (Ch. III.). The author thinks, however, that Spinoza was compelled, "not by the exigencies of his system, but by the logic of facts, to reproduce in the microcosm the scheme of the macrocosm;" for "it

would be possible to show that the modes of the attribute of thought are not, after all, ideas, but human spirits." The opposite or empirical dissolution of personality in empiricism is next developed (Ch. IV.). Here Locke's distinction between personal identity and identity of substance is emphasized, and the writer says, in his enthusiasm, that "Kant himself could hardly have defined the transcendental *ego* of apperception in better terms." "Berkeley rises to the higher thought that there is no true unity save in personality." The reference is to *Siris*, but "already in his commonplace book Berkeley writes: 'Nothing properly but persons, *i. e.*, conscious things, do exist. All other things are not so much existencies as manners of the existence of persons.'" Berkeley further recognized the essential activity of personality, thus anticipating the new period in the history of the doctrine inaugurated by Leibnitz. "In most of the earlier systems the chief emphasis was laid upon man as a thinking being. That man is also an acting being was added as a subordinate fact." In the new period "the standpoint was reversed, and man was regarded first of all as an active being. Personality was removed from the sphere of thought to that of action." This new standpoint is that of Kant in his doctrine of the Practical Reason, to which the last chapter of the thesis is devoted, and the relation of which to his doctrine of the Pure Reason is admirably shown. Kant is also "the precursor of a new period in which the discussion of the personality of man and the personality of God go hand in hand." To this post-Kantian period, presumably, the second part of the dissertation is devoted.

JAMES SETH.

CORNELL UNIVERSITY.

Our Notions of Number and Space. HERBERT NICHOLS, assisted by WILLIAM E. PARSONS. Boston, Ginn & Co. 1894. Pp. vi + 201.

Dr. Nichols has based upon a series of experiments performed at Cambridge certain views relating, as the title of his book shows, to the origin of our number and space judgments. His book thus presents not merely a statement and explanation of experimental results, but an apparently complete theory, comprised in no fewer than one hundred and nine articles, on the general subjects of space and number—a weighty superstructure to be founded on psychological experiment in its present stage of development.

The experiments from which Dr. Nichols derives his hypothesis were performed with simple apparatus, consisting of rows of pins fixed in cardboard, the number of pins in each row varying from two

to five, and the length of the rows varying by half-centimeter stages from one to five centimeters. Other experiments were performed with the same number and distance categories, but with the pins arranged in triangles, squares and circles. Cardboard edges of corresponding lengths were also used, and solid cork triangles, circles and squares for comparison with the pin experiments. Four persons served as reagents. The apparatus was applied by the reagent himself on the locality to be investigated, which was either the tongue, forehead, forearm or abdomen, and the pins were ordinarily 'rocked' back and forth on the skin to facilitate judgment. In the pin experiments the reagent was asked to determine in the same experiment both the number of pins and the length of the row; indeed, where the pins were set in triangles, *etc.*, 'figure' judgments were also required; a method which must have produced a certain amount of attentional distraction. A general criticism which suggests itself on a survey of Dr. Nichols' tables concerns his use of sub-liminal values. Many of his inferences are drawn from tables where the percentage of right judgments seldom rises above forty.

The chief results of these experiments may be briefly summarized as follows: (1) When two pins are used, the accuracy of both distance and number judgments increases with increasing distance. (2) When five pins are used, on the other hand, the closer together they are placed, the more accurate is the judgment. This result as regards the estimation of number, Dr. Nichols accounts for by the fact that reducing the distance really increases the uncertainty, but that increased uncertainty means an increased tendency to assign the higher numerical categories, and hence, where the number of pins is actually a maximum, increased correctness of number judgment. Such at least is what the present reviewer makes out of the most difficult passage in the book, whose style is nowhere very clear. (3) The number judgments when the pins are set in triangles, *etc.*, are more accurate than when the same number of pins are placed in a straight line. (4) The number judgments are more accurate when four pins are set in a square than when these are set in an equilateral triangle of the same base. (5) The distance between the pins seems shorter when the pins are set in a triangle or circle than when they are set in a square.

Such being the more important inferences from the experimental results, let us see what the derived space theory is. Since the author's own summary of his doctrine occupies twenty-five pages, only its merest outline can be suggested here, but the gist of it seems to be

this: If a combination of nerve endings have been on the whole stimulated oftener together than separately, the resulting presentation will be numerical and spatial unity; if oftener separately than together, we shall have a lineal distance presentation. The length of the distance presentation will depend 'upon the average length of all the time series in which the peripheral line has through life been stimulated.' The further apart two points, the greater the probability of serial rather than simultaneous stimulation; hence the more accurate the distance presentation. If the successive stimulation is discontinuous, the result is a presentation of number. The further apart two points, the more likely they are to be stimulated discontinuously, hence the more accurate the number presentation. The greater the number of points stimulated within a given distance, the less the probability that those points have been previously stimulated discontinuously, hence the less accurate the number presentation. At the same time, the more points touched in a given distance, the more clearly the 'distance tendency' will be recalled, and the more accurate the distance judgment. Three points in a triangle would average more discontinuous stimulation than three in a straight line, and four in a square than four in a straight line; hence we find the number judgments more exact in the experiments with triangles and squares. The fact that number judgments in the case of the square are more accurate than in the case of the triangle is explained by showing that the diagonal points of the square are further apart than any two points on the triangle. In distance judgments with triangles, squares and circles, the sides of a triangle are under-estimated because each corner calls up not only the presentation of the distance between itself and the opposite corners, but the shorter distance presentations between itself and other points on the perimeter, the average 'distance tendency' being thus shortened. Similarly, the diameter of a circle is under-estimated because each end recalls the shorter distance presentations between itself and other points on the circumference. Obviously no such shortening influence is exerted on the sides of a square.

These, as briefly stated as possible, are Dr. Nichols's explanations of his results. There is no space for detailed criticism, but two general observations present themselves. First, it will be seen that the author reduces space to a succession in time. He can hardly mean that it is nothing more, yet he seems to imply that he is giving us a complete theory of space. In a note (p. 155) where he criticises Professor James's doctrine of 'crude extensity,' he expressly says: "Our every notion of extensity is wholly an expression of time extension. * * * I

should say that independently of time form there could be no feeling of extensity, while, theoretically, there might be big feelings and little feelings in the sense of more feeling and less feeling." It is surely superfluous to remark that 'time extensity' is not space extensity, and that successive stimulation of different nerve endings could never make them spatially distinct for consciousness unless each one gave a sensation *spatially* distinguishable from that of every other nerve ending. 'Crude extensity,' or a series of local signs in the sense of original spatial differences, one or the other we must have.

Secondly, it is undoubtedly true, as Professor James has told us, that no elements can be analyzed out of a complex presentation unless they have been previously experienced separately. But is it not rather dangerous to claim that two points will not be felt as two unless 'on the whole' they have been oftener stimulated separately than together? When one thinks of the simultaneous stimulation, practically continuous through life, of distant parts of the skin by contact with clothing, one hesitates to say that no points are locally distinguishable save those which have been stimulated oftener separately than together. Is it absolutely certain that the volar surfaces of the ends of the forefinger and thumb average more successive than simultaneous stimulations? Yet on the assumption that the *average* experiences of a peripheral nerve-ending determine the result of its present stimulation rests the whole of Dr. Nichols's theory.

MARGARET WASHBURN.

WELLS COLLEGE.

The Education of the Central Nervous System, a Study of Foundations, especially of Sensory and Motor Training. REUBEN POST HALLECK. New York and London, The Macmillan Company. 1896. Pp. 251.

The first four chapters of this work contain a popular statement of elementary facts in the gross anatomy of the central nervous system and in neural physiology. The work assumes that the cortical centers are developed (1) by the exercise of the particular senses whose afferent fibres terminate in them, (2) by practice in recalling sensory images. To these ends an early beginning in the systematic exercise of all the senses, frequent changes of environment, and care on the part of the instructor to exercise the pupil in recalling sensory images, are recommended. The familiarity of great writers with the sensory aspect of nature, as seen in their poetry, is cited to prove, as it seems, that mental superiority is based upon well developed cortical centers corresponding to the special senses. In Chapter X., *How Shakes-*

peare's Senses were Trained, the author's hostility to the study of books reminds one of Rousseau. "No one was ever educated by the study of words" (176). Against 'those who favor going to school chiefly for the purpose of studying books, 'the education of Shakespeare is held up. His superiority is believed to be due to the fact that "he had magnificent sensory training and made the proper motor responses thereto" (180). Chapter XI. emphasizes the necessity of suitable motor responses to sensory stimuli. The last chapter gives pedagogical application to the pleasure-pain theory of Marshall. A convenient index is added to the text.

The style is lucid and very popular, the print clear and the pages handsome. The author offers no new facts, either anatomical, physiological, or pedagogical. The evidence upon which he rests his theory is not the experience of educators, but rather certain assumed physiological facts gleaned from the work of others; while the shadowy evidence upon which some of these 'facts' rest is not mentioned by the author. The precise genesis and function of the 'association' fibres, *e. g.*, as well as the physiological parallel of memory, etc., are problems as to whose solution physiologists can as yet only guess; yet the author cites certain assumed solutions of them and of other psychophysical problems as evidence for his theory. Is the evidence adequate? We think not; and yet it may be true that pedagogical theory pays too little attention to sensory and motor training in the early life of children. The important question as to how the senses and memory are to be rendered the trained servants of a will which moves toward intellectual, ethical or other practical aims, is not touched upon by the author.

GUY TAWNEY.

PRINCETON, N. J.

The Art of Controversy, and Other Posthumous Papers. ARTHUR SCHOPENHAUER. Selected and translated by T. BAILEY SAUNDERS., New York, The Macmillan Co., 1896. Pp. 116. 90 cents.

The volume is a valuable accession to Mr. Bailey Saunders' library of Schopenhauer. The excellent English translations of the German master have preserved the thought and spirit of the original while in no wise, however, sacrificing the demands of clear, idiomatic English. In the transfer from one language to the other there is in all of these volumes a minimum of loss as regards the impression made of the author's mind and personality. *The Art of Controversy* is one of Schopenhauer's posthumous papers, though a small part of it was pub-

lished before his death in the chapter of the *Parerga* entitled *Zur Logik und Dialektik*. The opening chapter consists of a theoretical exposition of the distinction between Logic and Dialectic, the one a guide in the search for truth, the other a weapon to wield in the quest of victory. This is followed by a practical discussion of the ways and means of securing an advantage over an adversary in debate. This chapter has the significant heading of *Strategems*, and has a vein of irony running through it that gives added force to its many valuable hints. The other essays of this volume are on *Interest and Beauty in Works of Art*, *Psychological Observations*, *Wisdom of Life*, *Genius and Virtue*. The epigrammatic style, the sententious comments upon human nature, and withal an underlying strain of humor, concealed and yet pervasive, render these essays both interesting and suggestive.

JOHN GRIER HIBBEN.

PRINCETON UNIVERSITY.

Zur Theorie der Aufmerksamkeit. HARRY E. KOHN. Halle, Niemeyer, 1895. Pp. 48.

The main contention of this paper is that there is no real difference between the two states of attentive and inattentive consciousness. The difference is only one of degree, and attention is regarded as the intensity coefficient of consciousness. The essential features of attention are discovered in the process of apperception. Different stimuli strive together to possess the field of consciousness, and the victorious inhibit the force of the others, their superior intensity being felt in consciousness as attention. Every change in consciousness or consequent shifting of attention rises from the fusion of a perception-mass with an apperception mass, resulting from the strife of contending stimuli. The writer indicates also as one of the factors in attention, the susceptibility, both original and acquired of the inner nature to certain stimuli in preference to others, thus increasing or diminishing their intensity as the case may be and so further modifying attention. His theory is mainly an exposition of the Herbartian doctrine of struggle and inhibition, and partakes of a like vagueness and artificiality. The constructive portion of the paper is followed by a criticism of the theories of Stumpf, Wundt and James.

JOHN GRIER HIBBEN.

PRINCETON UNIVERSITY.

Dictionnaire de physiologie. CH. RICHTER. I. A-B. Paris, Alcan, 1895. Pp. 1044.

Strange to say there has not existed heretofore any special dictionary for physiology. It is a lack which M. Ch. Richet has attempted

to fill, with the help of a great number of physiologists of distinction, mainly French. The first volume is now ready, and we find that it treats of physiology in the largest sense. It includes all the sciences commonly known as physics, bacteriology, medicine, chemistry, therapeutics and psychology. To psychology a considerable number of interesting articles are devoted. We may cite Aboulia, Amnesia, Analgesia, Anæsthesia, Apperception, Attention, Automatism, etc. These articles are signed by Marillier, Janet, and Richet. They are quite in their place in a dictionary of physiology, but they would be considered too summary in a dictionary of psychology. It is difficult to give a general opinion of these different articles, for they are of very unequal value. Some—as Algesimeter, Agraphia, Audition coloreé—are curiously inadequate; while others—as Hearing—are treatises. But my general impression is that this dictionary is a very useful work, and that it will be of more service to psychologists than the ordinary dictionaries of medicine, in which psychology is altogether sacrificed.

A. BINET.

PARIS.

Alterations of Personality. ALFRED BINET. Translated by HELEN GREEN BALDWIN. With notes and a preface by J. Mark Baldwin. New York, D. Appleton & Co. 1896. Pp. xii + 356.

Hypnotism, Mesmerism and the New Witchcraft. ERNEST HART. New edition, enlarged. New York, D. Appleton & Co. 1896. Pp. viii + 212.

The appearance of Mrs. Baldwin's excellent translation of M. Binet's monograph, and of a second edition of Dr. Hart's essays, offers an opportunity of giving them mention in the pages of the PSYCHOLOGICAL REVIEW.

M. Binet's book is divided into three parts—'Successive Personalities,' 'Coexistent Personalities,' and 'Alterations of Personality in Experiments on Suggestion.'

The first part deals with Spontaneous Somnambulism, the exposition of which is based chiefly upon the cases of Félicité X. and Louis V.; and with Induced Somnambulism. The latter term is used to designate those secondary states in which the patient is vividly conscious, although they are not remembered in the primary state. Hypnotic states in the narrower sense are scarcely mentioned. The phenomena of post-hypnotic suggestion are then used to prove that the somnambulistic state can survive the reappearance of the normal consciousness, thus constituting a secondary personality.

The conception thus introduced is worked out in detail in Parts II. and III. In Part II. it is used to explain the phenomena of hysterical amnesia, anæsthesia, automatic movement and automatic ideation, while in Part III. the analogous phenomena which can be produced by direct suggestion are interpreted in the same way, including in the latter class 'spirit' writing.

Into the details of M. Binet's reasoning one cannot enter within the limits of a brief review. It is dominated throughout by the notion of a subconscious personality or self, and although he does not hold that this subconscious personality is to be conceived as invariably analogous to the primary self, that it exists in all persons, or that it enjoys a continuous existence, he frequently writes as if he held all these doctrines. There is, in fact, a certain lack of clearness in M. Binet's conceptions, of which his loose interchange of such words as 'unconscious' and 'subconscious,' 'personality' and 'consciousness,' is merely the exponent. Notwithstanding such blemishes, however, the book is an earnest attempt at synthesis in a new field, and as the author is one of the few who can say of the early researches in that field, '*quorum pars magna fui*,' his views are of weight.

Dr. Hart writes in a very different vein. Someone has said that if an Englishman be asked for an opinion upon a subject of which he is ignorant he may for a while be at a loss, but after a half hour's reflection will be found, not only in possession of an opinion, but ready to knock down anyone who fails to agree to it.

Whether this be true on the whole or not, Dr. Hart's mental attitude seems to be of somewhat the same type. In 1850 he began the study of hypnotism and soon satisfied himself that hypnotic states exist, that they are of subjective origin, that will or magnetism has nothing to do with their production, and that the subject is suggestible. He further concluded that hypnotic sleep is due to a reflex inhibition of the cerebral circulation. At that point his powers of assimilation seem to have failed and he has never advanced a step. This, he holds, is the sole and only true faith of hypnotism, which except a man hold faithfully he shall be damned scientifically. All further alleged discoveries are compounds of malobservation and fraud, and those who are engaged in foisting them upon the world are either knaves or fools, mainly, however, fools. For even Dr. Hart can be charitable in his way.

These views are expounded in the first two essays. The third narrates the author's *exposé* of the methods of Luys and de Rochas. It is good as far as it goes, and so also is the amusing and well written essay on 'The Eternal Gullible.'

The book may be of service to the general public in pruning away some of the absurd notions about hypnotism which are so common. Otherwise it is insignificant. The sole point of importance which Dr. Hart makes is that fraud on the part of the patient is a source of error never absent and seldom provided for, but he probably grossly exaggerates the extent to which this vitiates the results of careful observers. Furthermore, the intemperance of his language and his supreme self-confidence will discredit his work in the eyes of all who value cool thinking, justice and courtesy. W. R. NEWBOLD.

UNIVERSITY OF PENNSYLVANIA.

Causal-Nexus zwischen Leib und Seele und die daraus resultierenden psychophysischen Phänomene. HEINRICH METSCHER. Dortmund, Ruhfus. Pp. 179. (No date.)

The first part of this work reviews historically and critically the principal metaphysical theories of the relation of soul and body. All are found unsatisfactory, but no new solution is attempted. Instead, the author, falling back on the general psycho-physical formula as expressing the empirical facts, proceeds, in the second part, to discuss the more characteristic phenomena which exhibit bodily and psychical interdependence. The historical data are almost all at second hand; in the more psychological portions the chief authority is Wundt, cited from the second edition. Wundt is also referred to as one of the leading investigators after Flourens of localization of brain-function. None of the real investigators is mentioned except Flechsig, whose name occurs twice, spelled with an x. In the more metaphysical portions the arguments proceed from the assumption that soul and body interact and from the demand for a theory to explain the fact. No effort is made to unravel the metaphysical implications of this assumption. The author does not consider what light might be thrown on the problem by the application of the critical method, regarding soul and body as terms derived from distinct points of view in the organization of experience, and thence developing the principles on which the experience that yields these distinctions rests. Nevertheless, the book has sufficient merits as a popular exposition. The material is well arranged and the style clear. H. N. GARDINER.

SMITH COLLEGE.

Die Willensfreiheit. PAUL MICHAELIS. I.-D. Leipzig, 1896. Pp. 56.

Following five pages of introduction, in which the problem is stated, is a sketch of its historical development, four pages being de-

voted to the Bible, two and a-half to Greek Philosophy, and so on through the Middle Ages and Modern Philosophy till Kant, Kant and his successors being finally disposed of in a little over four pages. The more independent second half discusses the Law of Causality, the Will, Character, Transcendental Freedom, the Development of Moral Will, the Freedom of Moral Will, Ethical Consequences and Education in Moral Freedom, allowing on an average about three pages to each topic. One is reminded of the traditional first sermon, into which the young theologian puts all of the divinity he knows. One's impression of the work, however, improves on acquaintance. Herr Michaelis has evidently not told all that he knew; and while we must regret that he did not confine himself to fewer subjects and develop them in a way which would have made his work of real value as a contribution to the discussion of the main problem, it is fair to say that he has selected his points with good judgment and presented them so succinctly and clearly that one gets from his work as good an idea of what the controversy is all about as from many much more pretentious treatises. The question discussed is the old one of the *liberum arbitrium indifferencie*, the *possibilitas utriusque partis*. The author well brings out its ethical and religious bearings. The interest in the question, he says, is not as to whether, when at the cross-roads, we can turn indifferently to the right or to the left, but whether Hercules, at the parting of the ways, can of himself choose 'the narrow path' or must wait for assistance from above, that is from without. This possibility Herr Michaelis denies. Our choices are determined. The arguments are the familiar ones and are perhaps conclusive. Few at any rate would contend to-day for a choice without *motive* or deny that the motive essential to choice is determined by circumstances and character. A modern defender of free-will would be likely to say that the question is as to the relation of motive, circumstances and character to will and choice and to maintain that the former, so far from being external forces which determine the will, are elements of it, that choice is determined, to be sure, but self-determined, and that self-determination, as opposed to the mere capacity of transmitting foreign energy, constitutes a freedom whose forms are so various as are the forms of life. The analytic method of Herr Michaelis leads him to overlook the possibility that self-determination may be a valid category and hence, though explicitly recognizing the difference, to really think of psychical causality after the analogy of physical. When, therefore, he himself substitutes a conception of freedom for the older conception, it is not that of self-determination which he gives, but 'will consciously di-

rected to the moral ideal.' In proportion as the dependence of the individual is insisted on, the function of society is exalted. By nature the individual is unfree. Nor can he free himself. His freedom, *i. e.*, his good moral disposition, which Luther and Augustine believed possible only through divine grace, must come to him from society. The great function of society is to educate its citizens for moral freedom, and to this end belongs, among other things, the education of the criminal and a social-economic condition free from incitements to envy.

H. N. GARDINER.

SMITH COLLEGE.

GENETIC.

Psychic Development of Young Animals and its Physical Correlation. WESLEY MILLS. Parts I. to VI. From the Transactions of the Royal Society of Canada. Dorie & Son, Ottawa, 1894-5.

These papers by Professor Mills consist of a series of diaries of the earliest psychic life of dogs, cats, rabbits, guinea-pigs, pigeons and domestic fowls, followed by some comments. To determine whether a reaction is conscious or reflex, and if conscious, what kind of consciousness, is, of course, more difficult with very young animals than with mature ones. Professor Mills often seems duly impressed with the difficulties of his task, as when (p. 55) he speaks of apparent anger in a pup of 17 days old as possibly merely a reflex. However, he is inclined (p. 53) to believe that a pup of 26 days has a 'sense of fun or humor.' Yet I think it must require a vast deal of very thorough evidence to lead us to believe that so young a pup 'makes believe' in biting play, and thus distinguishes between real and unreal, and so becomes an actor and makes a fiction which is enjoyed as such. So, also the kitten 20 days old, which hisses when called 'puss,' 'puss,' expresses probably fear rather than 'surprise,' as Professor Mills interprets.

Some suggestive remarks are made on the tail-wagging and barking of the dog (pp. 54, 224). But Professor Mills surely goes too far in saying that tail-waggings are to the dog what 'words are to mankind.' Whether the tail is primarily or only secondarily an instrument of expression, and whether it is purposely used, as the voice seems to be, for expression, and how far, these are questions which can only be settled by very thorough observation upon the dingo or wild dog and upon higher breeds. Professor Mills notes that growling precedes barking and that both first occur in sleep. An

observation recorded (p. 213) suggests that the bark and growl may be differentiated from some middle tone; but the subject needs to be studied with all dogs from the dingo up to come to any certain result.

Professor Mills grants the cat more docility and gratitude than is often allowed. He is much impressed by its relatively long period of psychic immaturity—more than twice that of the dog—and also by its superior persistency, even when very young. This quality, I think, is hereditary, and, being implied in all lying-in-wait and stalking in the feral form, naturally appears very early in the domesticated cat. Professor Mill's observations of the chick practically agree with Professor Lloyd Morgan's. If there is any thing new it is with reference to the sense of support. That most new-born land animals—the prairie dog is a notable exception—evidence a strong reaction on being brought to the edge of a table is, indeed, a marked fact, but just how far it is reflex, and how far a real consciousness as 'sense of support,' is very difficult to make out. Professor Mills shows clearly that length of infancy does not of itself point to higher development. Thus, though the rabbit takes three times as long in coming to maturity as the cavy, yet both are then 'about on the same mental plane.'

While Professor Mills' observations are necessarily somewhat isolated and meager, they appear unbiased, and are of interest and value as a step in the right direction. Complete and exact observations can only be made by those who can give up their whole time to making a full record, day and night for the whole period, and who are thoroughly conversant with the species studied. A thorough method would doubtless lead to the discovery of some temporary and recapitulatory phases of great historical import for mental embryology.

HIRAM M. STANLEY.

LAKE FOREST, ILL.

VISION.

1. *Action de la lumière sur la rétine.* ED PERGENS. Annales de la Soc. Roy. des Sci. Méd. et Nat. de Bruxelles. V. (3). Pp. 33, 1896.
2. *Zur Theorie der Farbenblindheit.* A. FICK. Pflüger's Archiv, 64, 313-321.
3. *Absorption und Zersetzung des Schfärfpurs bei den Wirbeltieren.* ELSE KÖTTGEN und GEORG ABELSDORFF. Ztsch. für Psych. und Phys. der Sinnesorgane, VII., 161-184.

4. *Vergleichende Untersuchungen über Raum-, Licht- und Farbensinn in Centrum und Peripherie der Netzhaut.* DR. GUILLERY. Ztsch. für Psych. und Phys. der Sinnesorgane, XII., 243-275.

That the cones of the retina are much shortened under the influence of light was first discovered by Van Genderen-Stort in 1887. Pergens here confirms the observation, having made use of the latest methods of staining and preservation. The animal experimented upon was a fish (*Leuciscus rutilus*), and great pains were taken to secure conditions absolutely alike for different individuals as regards everything except exposure to light. After numerous experiments it was found that the best results were obtained by the rapid method of Golgi; sometimes the retina was colored during life by mixing methylene blue or the Biondi mixture with the water in which the fishes swam. The amount by which the retina is shortened under the influence of light was found to be from 170μ to 220μ ; this was almost wholly due to a change in the length of the cones. There was also found to be a diminution in the amount of chromatine in most of the layers of the retina, and especially in the external granular layer; the author considers that this layer constitutes a reserve of protoplasm and nuclein in direct relation with the functioning of the rods and cones. The forward movement of the pigment follows upon the contraction of the cones, and takes place very much more slowly than that.

2. Professor Fick here produces his 'theory' of color-blindness. He represents normal color-vision by three curves indicating the excitability of the three kinds of color substance in the different portions of the spectrum, and considers that, in the different species of color blindness, one or another pair of these curves become co-incident. He gives curves of different shape from those usually chosen, and finds some vague reasons for preferring them; he is apparently unaware that König's curves have been so chosen, that they *fit* both in the normal eye and that which is color-blind. It is, since König's immensely laborious measurements, no longer a *theory* that the sensations of the color-blind can be represented by a curve which is the union of two curves of normal vision—it is plain matter of fact; instead of being vague speculation it is a deduction from observations of the utmost refinement, *provided* the curves are those adopted by König—that is to say, provided the fundamental colors are the same as those chosen by him with special reference to their serving the color equations of both normal and abnormal vision.

3. The authors here describe, with more detail regarding methods, than in the brief paper presented to the Berlin Academy of Sciences,

the examination which they have made of the visual purple of a number of different vertebrates, namely, thirteen mammals, three birds, eight amphibians and twenty-seven fishes. They find, as we have before noted, that there are two distinct kinds of visual purple, with no intermediate stages, one which has a faint purplish tinge and is found in all fishes, and another which is not purple at all, but red, and which is found in all other mammals. They also announce the remarkable fact that in all the lower animals the visual purple fades out without going through an intermediate stage of yellow, or that man is the only animal in whom the visual yellow occurs at all.

4. The writer of this paper is concerned to show that the several functions of the retina do not all decrease with like rapidity in passing from the fovea to the periphery, and hence that it is necessary to attribute them to different elements, either anatomical or chemical. He determines with this intention the rate of diminution of the space-sense, the light-sense, and the color-sense.

By space-sense one ought to mean, in the most elementary analysis, the feeling of the *where-ness* of a given sensation of brightness-difference, just as the sense of locality on the skin is the sense of the position in space of a temperature-sensation, or some other skin-sensation; it does not exist as a sensation in the abstract, but merely as a *qualé* of another sensation. But Dr. Guillery maintains that the simple perception of a black point on a white surface, without reference to its place, is an exercise of the space-sense, and he gives a long and very ineffective argument to support this claim. He proceeds to determine the physiological point (the size of the just discernible retinal image) for different distances from the fovea, and then the size of the image cast by two points of different brightness when they differ so much as to be just perceptibly different if their image falls upon a single cone of the fovea. He finds that these two functions diminish *pari passu* in proceeding from the fovea the ratio for corresponding points of the retina is—that nearly constant, which does nothing to confirm his theory that they are not one and the same function. The color-sense is naturally a sense which diminishes with a different rapidity from either of the other two; but here the writer's observations would be of greater interest if state of adaptation of the eye had been attended to; there is no indication of his knowing the importance of that condition. His conclusion, in passing, that the sense for blue and yellow (and the sense for red and green) fade out together, respectively, is as nugatory as all the other proofs of this supposed fact; the most that can be de-

terminated is that a blue and a yellow, etc., can be found *such that* they fade out at approximately equal distances, but no effort is made to show that the colored papers that happened to be chosen were in any sense of equal value for sensation at the fovea. Of the 'normal' colors prepared by Hegg, for instance, the red and the yellow would not strike the plain man as at all deserving of the name. The writer is quite unaware of the significance of the recently discussed functions of the visual purple.

CHRISTINE LADD FRANKLIN.

BALTIMORE.

Das Einfachsehen und seine Analogien. SIGMUND REICHARD. Ztschr. f. Psych. u. Physiol. d. Sinnesorg. XI., 286-290. 1896.

This article merely points out that the phenomenon of single vision with identical points of the two retinae has analogies in the phenomena of single hearing with corresponding nerve-endings in the two organs of Corti, of single smelling with two organs, and of single contact-sensation when two points are stimulated which lie within a single 'sensory-circle;' and attempts briefly to devise a theory of the anatomical development of the retinae such as would increase the analogy of the visual with the tactile phenomenon.

Ueber geometrisch-optische Täuschungen. ARMAND THIÉRY. Philos. Studien, XI., 307-370; 603-620; XII., 67-126. 1895.

This is a thorough and important contribution to the study of geometrical optical illusions in general. It discusses all the various kinds, gives the results of careful measurements of many of them under various conditions, outlines and criticises the different theories that have been advanced to account for them, and attempts to establish the view that all of them—whether illusions of direction, of size or of curvature—are due to the conscious or unconscious influence of a perspective interpretation of the figures. The following remarks by Prof. Wundt are appended to this article:

"I recognize the great importance of perspective projection for these phenomena, but cannot wholly agree in regarding the perspective idea as the primary cause of the illusions. I believe rather that as a rule the perspective idea itself is to be regarded as the effect of other primary elements, especially of position and movement of the eyes, and it seems to me that the proof of this thesis is itself to be found to a large degree in the observations above recorded. I intend shortly to return to this subject in a special article in these Studien."

Further analysis of Thiéry's views will be delayed until after the appearance of Wundt's article.

E. B. DELABARRE.

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FEELING.

Recherches experimentales sur la Joie et la Tristesse. G. DUMAS.

Rev. philosophique, June, July, August, 1896.

In spite of the works of Darwin, Spencer, Wundt, Mantegazza, and many others on the emotions, we have, as yet, no adequate experimental researches on the question. With this lack in mind we turn to this work of G. Dumas, who proposes to study joy and sadness in the insane, from the physiological point of view, by registering the capillary circulation, arterial tension, heart-beat and respiration.

In his three rather long articles the author gives us his results. To understand them thoroughly we must recall two laws established by the physiologist Marey, to which Dumas continually returns for the interpretation of physiological phenomena: 1. Vaso-motor constriction, *i. e.*, the contraction of the arteries under the influence of the excitation of a vaso-motor nerve of constriction, produces an increase of tension in the blood-pressure and a slowing in the heart-beat, while dilatation of the arteries, under the influence of the vaso-motor nerves of dilatation, produces the opposite effect. 2. Increase of the action of the heart produces increase of tension; and the reverse.

The author distinguishes six different affective types. 1. *Joy with hypertension*; in general paralytics, the heart-beat is accelerated, respiration likewise, the tension is feeble, the arteries are in a state of dilatation. The author holds that the joy is produced by the dilatation of the arteries; this indeed, according to the law of Marey explains the rapidity of the heart-beat and the diminution of tension.

2. *Joy with hypertension*; in different sorts of insane patients who are very excitable: accelerated heart-beat and respiration, the tension strong, the arteries constricted or dilated. The author holds that in these cases the cause of the changes of circulation is central, in the brain, which excites the heart, and the heart increases the tension. The constriction of the arteries is an insignificant phenomenon.

3. *Sadness with hypertension*; heart and respiration are slowed, tension strong, with constriction. In this case it is the constriction of the arteries which dominates everything; it produces the strong tension and slows the heart.

4. *Sadness with hypotension*; phenomena as in type three, except that the tension is feeble. Here we must hold that the constriction of the arteries does not increase the tension, since the heart is too feeble.

5. *Sadness with hypotension, and acceleration of the heart.*

This occurs in the active sadness of melancholics: constriction, acceleration of the heart with hypotension. These symptoms seem paradoxical, since in spite of the union of causes which tend to increase the tension, it remains feeble. The author supposes that the heart is not excited, although it appears to be so, and that it empties itself incompletely.

6. *Moral pain, hypertension, acceleration of the heart, vaso-constriction.* These are almost the same symptoms as those of joy of the second type, except that the respiration is more irregular in this case.

This brief account will show that M. Dumas' story is systematic; but when we look at these types we see that some of them are artificially explained, as the second, fourth and fifth.

I may take advantage of this occasion to point out a physiological error made by M. Dumas and all his predecessors, even Lange himself, whose theories the author criticizes. It is well known that Lange, employing a very simple formula, held that sadness is connected with a condition of arterial vaso-constriction, and joy with dilation. It is likely that Lange was led by theory on this point: the theory that in sadness the vessels ought to contract because, in this case, blood would be drawn from the tissues, the temperature would be lowered with lack of blood, paling of tissues, etc.; and on the contrary, with vaso-dilation, in joy, the blood circulates more freely, the temperature rises, the skin colors up, vitality is augmented. But these theoretical views do not seem adequate, and it seems that the most favorable condition to circulation is neither dilation nor contraction of the arteries, but a state intermediate between these extremes. Yet I do not now insist on this point, expecting to return to it on another occasion.

The error which I wish to point out consists in attributing to vaso-constriction the loss of blood in the hand, coldness in the extremities, and discoloration of the skin. They are independent phenomena, which may exist with constriction or not. In order to distinguish, it is necessary to study the form of the capillary pulse. If we take an example of true vaso-constriction, such as is produced by the sudden stroke of a bell or by strong inspiration, we find that this constriction shows itself clearly in the form of the pulse-curve; the tracing descends, the curve takes on a smaller size, and its decrotism is less. If at the same time we study the arterial pulse we find that it changes its form in consequence of the effect of the constriction of the arteries upon the flow of the blood; the curve rises, and its decrotism dimin-

ishes. The effect is analogous to that which follows pressure on the artery under the sphygmograph. These are the signs of vaso-constriction, which is above all an active phenomenon.

Now, in many circumstances, for example in the fatigue produced by a day of uninterrupted intellectual work, we find the occurrence of coldness, lowering of temperature and discoloration of the tissues. The pulse is faint and difficult to take. If the phenomenon is very accentuated we get only a linear tracing, with no sign of pulse; the respiration is slow, the heart-beat less than usual. Does this indicate vaso-constriction? Is there in this case an active constriction of the arteries? If so it would seem very extraordinary—such activity of the vaso-motors in the midst of the general lowering of the organic vitality. As a matter of fact, the form of the capillary pulse, when it can be registered, gives quite a different indication. We find a feeble pulse, lessened decrotism very high up on the line of oblique ascension; in a word, a pulse indicating weakness, lack of blood and slow circulation.

It is not astonishing that authors heretofore who have described the physiological effects of emotion have confused these two very different conditions of the capillary circulation. The confusion is almost inevitable if the form of the pulse is not registered. Moreover, this slow pulse is very difficult to register without special tambours and adjustable membranes of the kind which we use in the laboratory of the Sorbonne.

In conclusion, I am able to formulate the following practical rule: When the extremities are cold, discolored, we can not conclude to vaso-motor constriction without study of the force of the capillary pulse, and if it is impossible to register this pulse we cannot conclude from this impossibility to the presence of constriction.

As far as I can judge from my own experiments; M. Dumas deals sometimes with slow circulation, and more rarely with true constriction. Consequently, as he does not take account of this source of error, all the effective types which he distinguishes must be revised. There remains from this work the general conclusion, which is very interesting, that in joy there is an acceleration of the heart action, and of respiration, while in sadness these two functions are made slower; but that in active sadness, sharp suffering, the symptoms are almost the same as in joy. This certainly does not lead to a theory of emotion, but it is a useful contribution to the study of the question.

A. BINET.

PARIS.

Zur Lehre vom Einfluss der Gefühle auf die Vorstellungen und ihren Verlauf. GUSTAV STÖRRING. Phil. Studien, xii., Heft 4, pp. 475-524.

Dr. Störring devotes the greater portion of his paper to a philosophical and introspective study of his question. The first part is given to showing that feelings affect ideas through attention. Like Ribot, he believes that feelings determine the fixation of an object in consciousness, and are the basis of attention. This holds both in normal and in abnormal mental life. The second part of the article treats of the influence of feelings on association and reproduction; an idea with a strong feeling-tone is more suggestive, and has, in turn, greater suggestibility. Similar organic sensations may associate two ideas in consciousness.

Störring describes, also, an experimental research into the influence of feelings on the voluntary muscles. After mentioning Féré's similar work, he reviews the experiments of Münsterberg, which showed that under the influence of pleasure (*Lust*) outward movements were made too large, inward ones too small, and, conversely, under the influence of displeasure (*Unlust*) outward movements were made too small, inward ones too large. Störring constructed an apparatus with which the fore-arm swung freely in a horizontal plane, the elbow resting in a cup. The hand carried with it a thin board on which there was an index moving over a graduated scale. This scale was arranged in an arc, with the elbow-cup as a center. Readings could be taken from the scale, or the apparatus could be made self-recording.

The subject was drilled, with closed eyes, in first moving his hand through an arc of 10 cm., it being stopped by a peg at the end; and then in repeating the movement, as accurately as possible, the peg having meanwhile been removed. This imitation-movement was found to have a positive constant error, but comparatively small. When, however, a pleasant or displeasing feeling-tone was created by placing in the subject's mouth raspberry juice or a solution of salt, the constant error, positive or negative, became quite large. Störring found, in the case of a pleasant feeling-tone, a positive constant error for flexion of the arm; in the case of a displeasing feeling-tone, a negative constant error for flexion, and a positive one for extension. These results are directly contrary to Münsterberg's. Störring seems less interested in a pleasant feeling-tone than in one that is displeasing, not giving any results of arm-extension in the former case. The work appears to have been carefully done throughout; but there seems to have been only one person tested. LEONARD B. McWHOOD.

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PATHOLOGICAL.

Le Moi des Mourants: nouveaux Faits. V. EGGER. *Revue Philosophique*, XLII., 337-368, October, 1896.

In reply to Dr. Sollier's criticism of the title under which M. Egger introduced the discussion of this subject (see this REVIEW, III., 236 and 454), the latter explains that the terms used were not medical but psychological; the 'mourant' is one who is such for himself, *i. e.*, who believes himself to be dying, and as to the 'moi,' it does not, he says, exist unless such an one, "already prepared by previous reflections on himself and in full possession of his faculties, resume his past either by a rapid series of recollections or by spoken or written formulas." He further explains that his object in studying the mental states of the dying was to confirm his theory of the *ego*—the *ego* is 'the total recollection,' 'the consciousness of the past as such,' etc. Hence his explanation of the vivid resurgence of memory-images in accident cases as a special illustration of the 'moi vif' analogous to the expressive utterances, of which he gives many examples, in which those about to die sometimes appear to sum up a whole life and character. In other words, the cause of the reaction is held to be logical, not pathological. M. Egger, therefore, while accepting Dr. Sollier's explanation of the beatitude commonly felt at a certain stage of the crisis, namely, that it is the direct consequence of the bodily insensibility, refuses to admit Dr. Sollier's further hypothesis, that the phantasmagoria of memory-images is an indirect consequence of this same bodily condition. He claims, in opposition, anæsthesia without hypermnesia, and, again, hypermnesia without anæsthesia. The last, however, he does not establish, at least for the accident cases, and the cases cited of conscious reflection on the past and reflective anticipations of the impending future on the part of dying persons in full possession of their faculties would seem to belong to a different class from that of the vivid panoramic vision in certain cases which Sollier's hypothesis sought to account for. On any theory, there must, of course, be some sort of an organized past to recur to; M. Egger's theory requires it to be that of a civilized adult. Hence cases like that of Charles Darwin, who, when a schoolboy at Shrewsbury, experienced, during a fall of some seven or eight feet, such an extraordinarily rapid succession of ideas as seemed, he says, to contradict the assertion of the physiologists, that each thought requires an appreciable amount of time—cases like these fall outside of the theory. Darwin, to be sure, does not directly tell us that his ideas were memories. But in one of the new

cases here reported we find a person three times in his life in mortal danger, and surveying with extreme rapidity his past in the first experience when less than ten years old, and in the first only. How does M. Egger explain this? He explains it by reference to a 'moi précoce' and distinguishes between the 'moi encombrant' of school-boys and 'what the psychologist calls a *moi*.' There is no doubt a place for the distinction; only in this connection, while it suggests the sort of self reacting, does it succeed in removing the form of the reaction itself, the hypermnnesia, from the need of a mechanical explanation or in disconnecting it from cases like that of Darwin above, and its suggested pathological associates? We think not.

H. N. GARDINER.

SMITH COLLEGE.

Periodische Depressionszustände und ihre Pathogenesis auf dem Boden der harnsauren Diathese. C. LANGE. Tr. into German from second edition by HANS KURELLA. Leopold Voss, Hamburg and Leipzig, Publisher. 1896. Pp. 52, including Appendix.

This paper was first read before the Medical Association of Copenhagen, in 1886. The second edition (1895) has an appendix of 13 pages.

The author singles out of the classes of diseases known as Neurasthenia and Melancholia a very frequent affection which he calls 'periodic depression.' Although in some respects like the first stage of Melancholia, with which it is erroneously identified, it differentiates itself from it in that (1) the patients have neither fixed ideas nor hallucinations; they never ascribe their suffering to external agents; (2) the periodicity is a constant feature, while in Melancholia it is rare; (3) not one of the many hundred cases studied by the author went further than the supposed first stage.

The most characteristic feature of the disease is an alternation of periods of depression with periods of usual moral tone. The periodicity varies greatly. Generally, the shorter the depression periods the more regular is their reappearance; in some cases the movements of the disease are so regular that the day of their coming can be safely predicted. In more than half the cases that came to his notice it is between the ages of 25 and 35 that the disease made its appearance. It does not show any preference for any particular class of people, but "it is almost powerless with individuals without hereditary taint." In almost every case the author discovered a bad heredity.

The symptoms are those of nervous depression: dullness, sleepiness (not incompatible with disturbed sleep), apathy, inertia. The patient can hardly set to work, but when he has once begun he may experience almost as much difficulty in stopping. Yet there is no appearance of reduced efficiency. He is joyless, affectionless; the expression sometimes used by him, 'geistige Steifheit oder Versteinerung,' mental rigor, describes well his condition. Sometimes anguish is added to the ordinary blank depression. The physiological symptoms have less significance. There is a general expression of fatigue and of sorrow. The patient loses flesh, and this loss is made more apparent by the flaccidity of the muscles. The digestive organs are somewhat sluggish. Menstruation seems neither to influence the disease nor to be influenced by it.

Concerning the pathogeny of periodic depression, the author found in every case, as well during as between the depression periods, a strongly marked tendency to the formation of an abnormal quantity of uric acid sediment. According to the theory he adopts, the uric acid acts directly on the elements of the nervous system to which it is carried by the blood.

In the appendix, Lange answers some criticism questioning the sufficiency of the non-quantitative method with which he established the pathogeny of the disease. He also points to some signs indicating that at last, 'out of the chaos of Neurasthenia, Periodic depression is coming to light.'

The least satisfactory part of this specification of a new type of nervous disease, supported by observations on about 2,000 cases, is the one concerning the symptomatic importance of the presence of an excess of uric acid in the urine of the patients and its supposed relation to the disease.

J. H. LEUBA.

WORCESTER, MASS.

NEW BOOKS.

Abhandlungen zur Geschichte der Metaphysik, Psychologie und Religions philosophie in Deutschland seit Leibnitz. L. STRUMPELL. Hefte, I.-IV. Leipzig, Deichert. 1896.

The Art of Controversy. A. SCHOPENHAUER. Translated by T. B. SAUNDERS. London, Sonnenschein; New York, Macmillan. 1896. Pp. vi + 116. \$.90.

- Genius and Degeneration.* W. HIRSCH. From the second German edition. New York, Appleton. 1896. Pp. vi+333. \$3.50.
- Notes médico-legales.* H. J. GOSSE. Geneva, George. 1896. Quarto. Pp. 30.
- Das Princip der Entwicklung.* H. DINGER. Jena, Kämpfe. 1896. Pp. v+75.
- Experimentelle Studien über Associationen.* G. ASCHAFFENBURG. Leipzig, Englemann. 1895. Pp. 95. (Sonderab. aus Kraepelin's *Psychologische Arbeiten.*)
- Über den Einfluss von Arbeitspausen auf die geistige Leistungsfähigkeit.* E. AMBERG. Leipzig, Englemann. 1895. (From Kraepelin's *Psychologische Arbeiten.*)
- Die Willensfreiheit.* P. Michaelis, Leipzig. 1896. Pp. 56.
- Paidologie: Entwurf zu einer Wissenschaft des Kindes.* O. CHRISMAN. Jena, Vopelius. 1896. Pp. 96.
- Abhandlungen zur Philosophie und ihrer Geschichte.* Edited by B. ERDMANN, Halle a. S. Niemeyer: I. *David Hume's Kausalitätstheorie.* P. RICHTER. 1893. Pp. 50. II. *Andreas Rüdiger's Moralphilosophie.* W. CARLS, 1894. Pp. 51. III. *Hume's u. Berkeley's Philosophie der Mathematik.* E. MEYER. 1894. Pp. 57. IV. *Thomas Hill Green und der Utilitarismus.* G. F. JAMES. 1894. Pp. 37. V. *Zur Theorie der Aufmerksamkeit.* H. E. KOHN. 1895. Pp. 48. VI. *Kepler's Lehre von der Gravitation.* E. GOLDBECK. 1896. Pp. 52. VII. *Der Unterschied der Lehren Hume's im Treatise und im Inquiry.* W. BREDE. 1896. Pp. 50. VIII. *Die motorischen Wortvorstellungen.* R. DODGE. 1896. Pp. 78.
- The Development of the Doctrine of Personality in Modern Philosophy.* W. H. WALKER. Part I. Ann Arbor, Mich., The Inland Press. No date. Pp. 79.
- Action de la Lumière sur la Rétine.* E. PERGENS. Brussels, Larmertin. 1896. Pp. 33.
- Die Entwicklung des Seelenbegriffes bei Kant.* M. BRAHN. Leipzig, Gerhardt. No date. Pp. 66.
- Causal-Nexus zwischen Leib und Seele.* H. METSCHER. Dortmund, Ruhfus. No date. Pp. 177.
- The Psychic Development of Young Animals and its Physical Correlation.* WESLEY MILLS. Parts II. to VI. Reprinted from the Trans. Roy. Soc. of Canada, Second Series, 1895-6, Vol. I., Sec. 4. Durie & Son, Ottawa. Pp. 191-252.

- Periodische Depressionszustände und ihre Pathogenese.* C. LANGE.
Translated by H. KURELLA. Hamburg and Leipzig, Voss.
1896. Pp. 55.
- Report of the Commissioner of Education, 1893-4.* Vols. I. and II.
N. W. HARRIS. Washington, Government Printing Office. 1896.
- Kategorienlehre.* E. v. HARTMANN. Bd. X. of *Ausgewählte Werke*. Leipzig, Haacke. 1896. Pp. xv + 556.

NOTES.

MR. R. P. HALLECK considers the review of his book in the last number of the REVIEW unfair to him. The reviewer, Professor Kirkpatrick, sends the following letter, in further explanation of his criticisms: "Inasmuch as Mr. Halleck thinks my review of his 'Psychology and Psychic Culture' unfair and even 'brutal' in its criticism of his error in regard to the psychophysical law, I wish to say that I do not believe that there are *many serious* errors in the book, for that was the only one found in a careful reading of a number of topics. That one was of such a nature, however, that no careful reader of modern psychology could have made it, hence one cannot be sure that the rest of the book is reliable without a careful examination of every sentence. I may also emphasize the fact that the author has succeeded in his aim of making a clear and interesting text book in which many of the illustrations are peculiarly apt, and that he is to be criticised for his subordination of other things to that aim rather than as to the way in which he has carried it out."

AN EXPLANATION.

Prof. H. C. WARREN's appreciation of my *Outline of Psychology* is so generous, and the tone of his criticism so uniformly moderate, that I hesitate to offer objection to any of his statements. Indeed, for his remarks upon the scientific aspects of my work I cannot but be grateful. As regards what I may call an ethical aspect of it, however, he is so unfair to what was at any rate my intention that reply seems called for. He says: "Careful search fails to reveal a single reference to modern psychological literature in the whole book. This is certainly a most singular omission and is much to be regretted. * * * (The book) takes no pains to direct into proper channels the desire

for further reading which it will undoubtedly provoke." I have said in my preface (p. vi.): "The system * * * stands * * * in the closest relation to that presented in the more advanced treatises of the German experimental school, Külpe's *Outlines of Psychology* and Wundt's *Grundzüge der physiologischen Psychologie*. While I have tried to make the present work complete in itself, I have also written with the view of producing a book which should be preparatory to these standard psychologies." I have thus explicitly directed the reader to two modern hand-books of psychology, in both of which he will find copious literary references.

E. B. TITCHENER.

Prof. TITCHENER's remarks are quite true, and I thank him for pointing out the ambiguity in my statement. What I alluded to were page references, for further reading on special topics; I think this evident from the context. My criticism here was intended to be entirely *practical*, not ethical.

H. C. WARREN.

DR. G. A. TAWNEY, Princeton, has been appointed to the chair in philosophy in Beloit College, Wisconsin, made vacant by the death of Prof. Blaisdell.

THE REVIEW has received Prof. C. Stnmpf's diagrammatic *Tafeln zur Geschichte der Philosophie*. They will be found helpful in the teaching of the history of philosophy. (Berlin, Speyer & Peters, 1896, 80 Pf.)

SCHLEICHER FRÈRES, Paris, announce as in press the first issue (for 1895) of an *Année Biologique* described as *Comptes Rendus annuels des Travaux de Biologie générale*, directed by Prof. Ires Delage, of the Sorbonne.

A *Philosophisches Lexikon*, edited by Dr. M. Klein, with the assistance of a number of writers, principally German, is announced by Reissland (Leipzig). It is to be issued in 25 parts (M. 2.40 each). We note the name of Prof. M. M. Curtis, of Cleveland, Ohio, among the contributors.

PROFESSOR FLOURNOY, of Geneva, has published a *Notice sur le Laboratoire de Psychologie de l'Université de Genève*, on occasion of the National Swiss Exposition. It contains lists of apparatus and publications, and interesting remarks on experimental psychology.

